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THE

QUARTERLY JOURNAL

OF

SCIENCE,

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HOMERTON
COLLEGE
SOCIETY.

JULY TO DECEMBER, 1829.

18

LONDON:

HENRY COLBURN AND RICHARD BENTLEY,
NEW BURLINGTON-STREET.

MDCCCXXX.

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TO OUR READERS AND CORRESPONDENTS.

The best answer to F. R. S. is the insertion of Dr. Wollaston's paper.

All that is important in Mr. R.'s paper has been said fifty times by other persons. We cannot undertake to forward communications to other journals, and request in future that such letters may be franked.

We have been obliged to postpone the insertion of the Wiseman Papers, in consequence of the lamented death of our correspondent, Mr. Wadd.

We must decline the letter on Naval Affairs.

We have seen Dr. Graham's Catechism: it is not so good as Parkes's; and what is worse, is a piracy upon the title of that deservedly popular work.

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TO OUR READERS AND CORRESPONDENTS.

We have received several communications upon the Supply of Water to the Metropolis, which will be noticed in due season. Mr. Philipson's plan has also been received, but we are certain that no source but the Thames can be safely resorted to. We quite agree with "an Old Subscriber" respecting the carelessness with which the basin in the Green Park appears to be treated, but the story of the infusion of dead bodies is exaggerated.

Filtration must lessen the evil, and therefore *is* an improvement.

The copy of "Berzelius" has reached us, for which we are much obliged.

A communication from Glasgow reached us too late for the present number.

We have received two "additional letters" upon the subject of burials in the streets of London; but as it scarcely comes within our province, we cannot see the use of disgusting our readers with the "narrative."

We are always happy to be corrected by F.R.S., but cannot take up the subject he alludes to, after the trash that has been published.

It was *erroneously* stated in our last number, that Dr. Marshall Hall had been anticipated in his views on the Mechanism of Vomiting by M. Richerand. Those of our readers who are interested in this question, will be satisfied of the correctness of our present statement by referring to the Medical Gazette, No. 56, for December the 27th, 1828, p. 127; and No. 58, for January the 10th, 1829, p. 189.

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THE

QUARTERLY JOURNAL

OF

SCIENCE, LITERATURE, AND ART.

On M. Hansteen's recent Magnetic Observations in Siberia.

In a Letter to Professor RENWICK of New York.

Somerset House, July 20, 1829.

MY DEAR SIR,

I RECEIVED a few days ago a letter from Professor Hansteen of Christiania, dated from Irkutsk in Siberia, in April last. M. Hansteen is travelling, as you know, at the expense of his King, and with the permission of the Emperor of Russia, for the purpose of observing the Magnetic Dip, Variation, and Intensity, over the whole of the north of Europe and of Asia; and of comparing the actual phenomena with the system of terrestrial magnetism propounded by himself, in his celebrated treatise entitled "Magnetismus der Erde."

The observations that M. Hansteen has already made in the first year of his undertaking, and the conclusions which they establish in regard to the directions assumed by the isodynamic curves, or curves of equal magnetic intensity, are in the highest degree curious and important. In the letter with which he has favoured me, he has taken the trouble to communicate his observations in full detail, and has expressly permitted me to make every use of them that I may think proper, "especially when it may encourage to new undertakings and accordingly forward the science." Having been requested by you to superintend the construction in this country of a part of the magnetic instruments, designed for the expedition now preparing

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by the government of the United States, for scientific researches in the southern hemisphere, I cannot anticipate a more favourable opportunity of turning to good account the information of which M. Hansteen has so liberally made me the depository. Since analogy would lead us to expect that a corresponding system of magnetism prevails in the two hemispheres of our globe, a knowledge of the arrangement of the system in the northern hemisphere may prove an important guide and direction for corresponding researches in the southern; whilst the example of M. Hansteen's undertaking may stimulate, and his success is well calculated to encourage, those who are about to enter on a career honourable alike to themselves and to the government under whose instructions they are employed.

For some years past it has been the opinion of several persons who have attentively considered the subject, that a knowledge of the general system of the magnetism of our globe is more likely to be attained by experiments on the relative intensity of the magnetic attraction in different parts of the earth's surface, than by observations on the Dip, or Variation, of the needle. In conformity with this opinion, M. Hansteen (without, however, neglecting to observe on all occasions the three phenomena conjunctively) has applied himself especially to trace the lines connecting those places on the globe, where a needle freely suspended in the magnetic direction, and drawn a certain number of degrees from rest, is found to make an equal number of vibrations around its point of rest in a given time. It was to be expected that these lines of equal intensity would arrange themselves systematically round the point or points in each hemisphere where the intensity was greatest: and, on the supposition that two such points would be found opposite to each other on the globe, one in the northern and the other in the southern hemisphere, that the isodynamic lines would form parallel circles, analogous to those of geographical latitude, progressively diminishing in intensity from the two points of maximum or poles, to the boundary circle of the two hemispheres, which circle, following the same analogy, might receive the appellation of the Magnetic Equator. Such was in fact the system, which, until the decisive discoveries which

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M. Hansteen has now made, appeared sufficiently conformable to the existing observations to receive their countenance and support. It had so happened that the previous observations, although extending widely over the magnetic parallels in the northern hemisphere, namely, from the least almost to the greatest intensity, were confined in respect to longitude to a space little more than the quarter of a hemisphere; and to that quarter which is immediately opposite to the countries visited by M. Hansteen. Within the space that had been thus examined, the isodynamic curves appeared to arrange themselves, with comparatively insignificant deviations, in parallel circles, around a point situated in the north-eastern part of Hudson's bay, and, as nearly as could be judged, about the intersection of the 60th degree of geographical latitude with the meridian of 80° west of Greenwich. That a system apparently so simple, so like the arrangement of induced magnetism in a sphere of iron, and corroborated by the approximation of results observed over a fourth part of a hemisphere, should have been viewed as likely to prove eventually the general system of the globe, is not surprising. It is the peculiar distinction of M. Hansteen to have been led by a more careful consideration of the slight apparent deviations which have been noticed, and of the general disposition on the globe of the lines of Dip and Variation, to infer the existence of a second point of principal magnetic action in the northern hemisphere: a fact, which by his recent observations must now be regarded as fully established; the isodynamic curves being found to arrange themselves systematically around two points, one in Hudson's bay and one in Siberia; and to be governed in the courses which they follow, partly by their distances respectively from those points, and partly by a disparity in the absolute attractive force at the points themselves, the maximum intensity in Siberia appearing to be weaker than the maximum in Hudson's bay.

The accompanying sketch of the northern hemisphere may enable me to convey a more distinct notion of the arrangement of the isodynamic curves than could be done by description alone: the portions traced in unbroken lines mark the connexion between places at which an equal intensity has been

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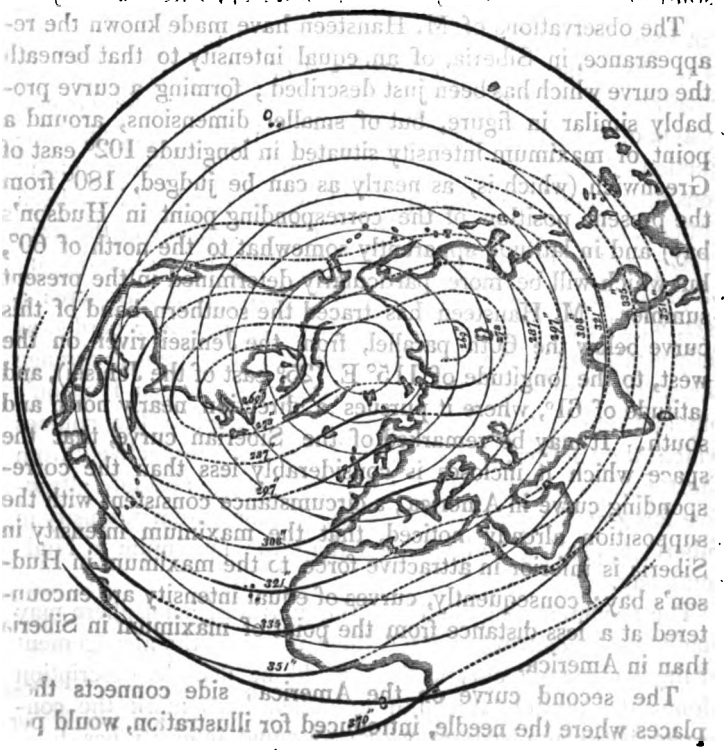
observed; and those in dotted lines exhibit the supposed completion of the curves, in parts of the hemisphere where the intensity has not been as yet examined. The portions which arrange themselves around the point in Hudson's bay are chiefly laid down from observations made by myself in two voyages of North-west discovery, those of 1818, and of 1819-1820;—in a voyage in 1822 to the equatorial shores of the Atlantic, and to several of the islands in the Atlantic and Caribbean seas,—and in a fourth voyage, in 1823, to Greenland, Spitzbergen, and Norway. Their prolongations around the point in Siberia are from the recent observations of M. Hansteen and the gentlemen who accompany him. A brief notice of each of the curves in succession will enable me to point out generally the places which have furnished their respective authorities.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Commencing with the intensities of the highest order, the curve drawn through the countries surrounding Hudson's bay is laid down from observations made at occasional intervals, from Regent's inlet in the north-west quarter, by Baffin's in the north, to Davis' strait in the north-east; and again at New York in the south. In places situated under this curve a needle, freely suspended, which required 300 seconds to perform a given number of vibrations (designated by n) in London, would perform the same number of vibrations (in integer numbers) in 269 seconds. In the space included by this curve, within which, except at New York, no observations have hitherto been made, it may be presumed that the intensity progressively increases until it attains its maximum at a central point, for the observations made in receding from the curve in different directions, namely at Melville island, in Greenland, and to the southward of New York, all manifest an opposite tendency.

The observations of M. Hansteen have made known the reappearance, in Siberia, of an equal intensity to that beneath the curve which has been just described; forming a curve probably similar in figure, but of smaller dimensions, around a point of maximum intensity situated in longitude 102° east of Greenwich (which is, as nearly as can be judged, 180° from the present position of the corresponding point in Hudson's bay) and in latitude apparently somewhat to the north of 60°, but which will be more particularly determined in the present summer. M. Hansteen has traced the southern band of this curve below the 60th parallel, from the Jenisei river on the west, to the longitude of 115° E. (25° east of the Jenisei), and latitude of 61°, where it pursues a direction nearly north and south. It may be remarked of the Siberian curve, that the space which it incloses is considerably less than the corresponding curve in America; a circumstance consistent with the supposition already noticed, that the maximum intensity in Siberia is inferior in attractive force to the maximum in Hudson's bay: consequently, curves of equal intensity are encountered at a less distance from the point of maximum in Siberia than in America.

The second curve on the American side connects those places where the needle, introduced for illustration, would per-

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form 1 vibrations in 278 seconds. The points which have determined it are, Melville island, in the north-west; several stations on the west side of Greenland from lat. 76° to lat. 60° in the north-east; and finally, a greater intensity observed at New York and a lesser at the Havanna; whence it is concluded that this curve intersects the seabord of the United States at an intermediate point between those cities. A corresponding intensity has been traced by Dr. Erman of Berlin (who accompanied M. Hansteen to Siberia) from the mouth of the river Oby, in lat. 68° and long. 70° E., preserving nearly the direction of a meridian to lat. 60°, where it bends gradually to the eastward, passes between Tobolsk and Narym, and has been observed at Kaihsk, by M. Hansteen, on its way to its probable southern limit on the Atlantic side, a few degrees south of lake Baikal.

The third curve is that in which the needle would perform 1 vibrations in 287 seconds. It is laid down from observations, 1st, at the Havanna; 2d, at the Pendulum islands on the eastern side of Greenland, in latitude 74°.5, where a somewhat greater intensity was found; and 3d, between Hammerfest near the north cape of Europe, and Spitzbergen. By M. Hansteen's observations it enters the continent of Europe between Archangel and Nova Zembla, and was crossed by him on the route from Moscow to Tobolsk, in 56° and 57° east longitude, and 57° and 58° latitude.

The fourth curve is that in which the needle would make 1 vibrations in 297 seconds. Its tracing from observation commences, on the American side, near the island of Jamaica, where the time of vibration was 294 seconds. Crossing the Atlantic, it passes through the northern parts of the British islands, and enters Norway south of Bergen. It there became subject to M. Hansteen's observation, who has ascertained its northern limit (from whence it begins to bend to the southward) to be on the shores of the gulf of Bothnia, midway between Stockholm and Tornea. He has since traced its prolongation through St. Petersburg and Moscow.

It is M. Hansteen's intention to commence the present summer by descending the Jenisei to Tourouhansk under the polar circle, in order to extend the tracing of the curve of greatest

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intensity; to return to Krasnojarsk, and to cross, in a route from thence to the Caspian sea, the curves 278, 287, and 297, in their further prolongation to the south-east: whilst Dr. Erman, who quits him at Irkutsk, and is furnished with the necessary instruments, will proceed by Jakutsk and Ochotsk to Kamtchatka, in which route he expects again to cross the same curves, after they have passed their southern Asiatic limit, and resumed for a second time a north-easterly direction.

These are all the curves of which M. Hansteen has ascertained the reappearance on the Asiatic side, those of lesser intensity passing altogether to the south of his present journey. I shall however briefly notice the remainder, in order to complete the sketch of the isodynamic curves in the northern hemisphere, as far as the observations will warrant. The next curve, in which the needle would make n vibrations in 308 seconds, was observed by M. Humboldt in 1800-1805, to pass near the cities of Mexico and Carthagena; by myself in 1822, near Teneriffe; and again by M. Humboldt at Madrid and in the south of France. The next, in which the needle would require 321 seconds for n vibrations, was observed both by M. Humboldt and myself on the South American shore of the Atlantic, near the 10th degree of north latitude; and by myself was ascertained to pass to the north of Port Praya in the Cape Verd islands. The next, in which the needle would make n vibrations in 335 seconds, was frequently observed by M. Humboldt in the interior and on the western side of Columbia. After crossing the Atlantic, it enters the continent of Africa somewhat to the south of the Gambia river, as is shewn by my observations at Bathurst, where the intensity was greater, and at Sierra Leone where it was less. The next, where the needle would require 351 seconds for n vibrations, was observed by M. Humboldt at Tompenda in Peru, on the western side of South America; at Maranham on the eastern side by myself; and on the African side of the Atlantic it enters the continent of Africa, south of Sierra Leone. Finally, the curve of least intensity which appears in this quarter of the northern hemisphere, is that in which the needle would require 370 seconds for n vibrations: in its progress from the southern hemisphere, (where it was observed by myself at Bahia and Ascension), it

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crosses the Equator near the western coast of Africa, as shewn by my observations at the island of St. Thomas.

We may hope that the further tracing of the curves, in the Asiatic quarter, which have not been subject to M. Hansteen's observations in Siberia, will ere long be accomplished by the scientific industry of British officers employed in India; where a line through the British dominions, from Ceylon on the south, to the Himalaya mountains on the north, would probably intersect the curves designated respectively by 308, 321, 335, and 351 seconds, nearly at right angles to their course.

Mr. David Douglas, well known to you as the enterprising traveller and successful naturalist, in the countries adjacent to the Columbia river and its tributaries, returns in September to the north-west coast of America, on an undertaking which will occupy him there many months. He will be well provided with instruments, and is practised in the modes of observation. He hopes to determine the magnetic phenomena from California in the south, to the furthest extent towards the north to which circumstances may enable him to prosecute his researches; and from the ocean on the west, occasionally to the Rocky mountains on the east. He will probably ascertain the situation on the western side of North America of the curves 287″ and 297″, and will approach 278″, when at his eastern limits. But it is from travellers in the interior of the United States, and in the countries adjacent to the Slave lake and Coppermine river, that we must expect exact determinations of this interesting curve 278″. Unquestionably, however, the space included by the innermost curve is the field for observations of the very highest importance on the subject of the magnetism of the globe; and as it is traversed annually under the direction of the Hudson's bay Company, we may confidently hope, from the ready disposition which that Company has shewn, in so many instances, to promote scientific researches, that much time will not elapse before that really important journey will be performed by a person properly qualified, by previous practice, to observe with the precision necessary on so particular an occasion.

In regard to the great space in the northern hemisphere occupied by the Pacific Ocean, the numerous islands with which

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it is interposed present points of observation of easier access than many parts of the respective continents. A commencement has already been made by Captain Lütke, commanding one of the Russian ships of war at present engaged in a scientific voyage. In a letter which I have received from him, dated from New Archangel (Norfolk Sound) in July, 1827, he has been so obliging as to communicate to me the results of several observations on the Magnetic Dip and Intensity, which he had found opportunities of making in his passage from Conception. I have not availed myself of these in the accompanying sketch of the isodynamic curves, because I regard his communication as private until he shall have returned and made his own observations public. At the date of his letter he was on the point of sailing for Behring's strait and Kamtchatka, which voyage, as well as in his subsequent operations, he will doubtless have obtained results of great interest.

If we now direct our attention to the southern hemisphere, we find nearly the whole field of enquiry untrodden. Of published observations, there are only those made by M. de Rossel, in the voyage of D'Entrecasteaux, at Java, Amboyna, and Van Diemen's Land. Of observations made, but not yet published, there are, 1st, those of Captain de Freycinet, at several stations visited by the expedition under his command: of these no public account has yet, I believe, been given: 2d, those which are at present in progress by Captain King, who is engaged in the survey of the southern parts of South America. The results obtained by him in the first year of his survey have been received in England: they commence at Rio Janeiro, and are continued at intervals down the eastern coast as far as Port Famine: he will probably have since extended them to Conception on the western side, the limit of his survey in that quarter. The results transmitted will require some slight modifications on his return to this country, to compensate for differences of temperature, &c.: but none that can interfere with their general effect in evidencing a progressively and rapidly increasing intensity, from the neighbourhood of Rio, where it corresponds with the curve marked 370″ in the accompanying sketch, to the Straits of Magellan, where it is intermediate between the intensities designated by 278″ and 287″. The

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observations of M. de Rossel indicate in like manner that at the period of his voyage, towards the close of the last century, the several intensities, from that represented by 370″, to that represented by 278″, were all comprised between Java in the north-west, and Van Diemen's Land in the south-east. Hence, as far as the evidence hitherto extends, it would appear that there are two points of maximum intensity in the southern as well as in the northern hemisphere: but the geographical position of those points, and their respective intensities, relatively to each other, and to the points of maximum in the northern hemisphere, remain to be determined, and must be acknowledged to be subjects of highly curious and important enquiry. In the arrangement of magnetism, as exhibited to us on the great scale of our globe,—differing, as it is now known to do, so widely from the analogies with which it had been associated, and indeed, I believe, from all analogy whatsoever with which we are acquainted,—we cannot too soon inform ourselves accurately of the facts.

In selecting the parts of the southern hemisphere in which enquiries of this nature may be most advantageously pursued, regard must be paid, in the first instance, to the distribution of land, on account of the convenience which its coasts and islands afford in determining and connecting the isodynamic curves. The eastern and western coasts of New Holland, and the adjoining island of New Zealand,—the western coast of South America from Lima to Cape Horn, and a continuation to the lands to the southward of Cape Horn approaching the Antarctic Circle,—the islands which might be successively visited in a course from the Cape of Good Hope to Desolation Island, and from thence to the Mauritius,—present in this view the directions of principal interest. Careful observations systematically made in them, combined with the observations already made, would advance our knowledge of the magnetic phenomena of the southern hemisphere to the same stage that it has attained in regard to those of the northern: viz. it would establish the number of the governing points of intensity in the hemisphere; determine their respective geographical positions, and, in great measure at least, their relative intensities; ascertain the general arrangement of the curves; and, finally, point out

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those localities of peculiar interest, which it might be expedient to visit for more particular enquiry. A single expedition might accomplish all this, without extending the duration of the voyage to an undue length, or interfering with other important objects of scientific research; and we may assuredly affirm, that were this service the single purpose, and sole object accomplished, by a scientific expedition, it would of itself confer no ordinary distinction.

In what has hitherto been said, observations made on land have alone been taken into account: the motion of a ship, and the quantity of iron necessarily employed in her equipment, impeding the prosecution of such researches at sea, and presenting embarrassments which, to say the least of them, are very difficult to surmount, and but too likely to impair the accuracy of the results. Still, when we consider how large a portion of the southern hemisphere is covered by the ocean, it does appear desirable to make the endeavour to obtain the best results, that circumstances will permit, over such extensive portions of the globe; and particularly as in the opinion of those, who from experience are most competent to judge, it is possible, by great care, to obtain results worthy of confidence. M. Humboldt has recorded several observations which he made himself at sea, in the northern Atlantic, both of the Dip and of the Intensity, the latter of which accord well with the curves of intensity traced in the accompanying sketch. M. Hansteen believes, that by giving a dipping needle the sort of suspension used in Captain Cook's third voyage,—by choosing those times for observation when a calm sea and moderate wind allow the ship to keep a steady course,—by confining the use of the instrument always to the same place on the ship's deck,—and by comparing the results on board and on shore on all occasions, when in harbour,—observations on board ship might become very valuable. I will venture to add an extract on this subject from Captain Lütke's letter, whose remarks are the more encouraging as, when he quitted Europe, he was by no means sanguine of success in the use of delicate magnetic instruments at sea. "Je dois pourtant faire quelques remarques sur les observations faites à bord. J'avais été, comme vous, en doute qu'elles puissent donner des résultats dignes de confiance; mais

l'expérience m'a appris qu'en faisant choix d'un endroit assez éloigné de toute grande masse de matière ferrugineuse, on peut atteindre à une précision suffisante. Dans toutes mes recherches je n'ai jamais manqué de comparer les indications des aiguilles à bord, à celles de terre. A Bio et à la Conception les résultats ont été à peu près identiques; ici (at Norfolk Sound) où l'inclinaison comme la force ont atteint leurs maxima (c'est-à-dire pour nous) et par conséquent où l'influence du fer sur les aiguilles — *materia peccans* — est aussi à son maximum, ici l'inclinaison à bord ne différait de celle à terre que d'un petit nombre de minutes; l'intensité de la force indiquée par l'aiguille verticale fut précisément la même, et d'après l'aiguille horizontale un peu moindre. En considérant que l'épreuve se fait par les circonstances les plus désavantageuses, on conclut naturellement que les observations faites à bord méritent quelque confiance. Mais il est essentiel de mettre toute l'attention possible au choix du propre endroit; car voulant faire les observations dans ma chambre, je n'étois parvenu qu'à des résultats très fautifs.

For experiments on the Magnetic Force, it is of the first necessity that the needles employed should retain throughout the same degree of magnetism; or should undergo merely such slight and gradual alterations in that respect, as admit of corrections being applied by interpolation, from experiments made at the same spot before and after the series in which they have been employed. This property of the needles ought always to have been ascertained by previous trial during several months. Those which I send you belonged originally to Mr. Hansteen, and have been in my possession, and in constant use, for three years past: their magnetism has hitherto undergone a slight but very regular diminution from year to year, well admitting of interpolation. It will be proper, therefore, that observations should be made with them at the port from which the expedition sails, a few days before its departure, and again in the same place, as soon as convenient after its return. It will also be proper that the needles should be then sent back to London, that observations may be repeated with them here, to ensure the connexion of the results obtained by their means, with those of the other experimentors which regard London, Paris, and

Christiania, as their base. The needles should be kept apart from each other, and from contact with iron, and particularly with magnetised iron.

I do not attempt in this letter to enter at any length on the consideration of the curves of Dip and of Variation. M. Hansteen has shewn, in the treatise already named, the general conformity of these phenomena to such an arrangement of magnetic attraction, as is indicated by the course of the isodynamic curves. His observations in Siberia, in so far as they go, confirm this view. Thus, for example, in the parallel of 55° north, the Dip, which in tracing the parallel to the eastward progressively decreases from Labrador, where it exceeds 80°, is found by M. Hansteen to attain a minimum of 67°½ about the 42nd degree of longitude east of Greenwich; from thence it increases, until the intersection of the parallel with the meridian of the Siberian maximum of intensity (102° E.), where its amount is 70°½: from that meridian it again decreases to a second minimum, by the observations of Russian officers, in the meridian of Kamtchatka (163° east). Hence, as regards the dip in the parallel of 55° N., there are two points of maximum and two of minimum; those of maximum are in the same geographical meridians, or nearly so, as the points of maximum intensity, and those of minimum occur respectively in meridians 120° on either side of the Hudson's bay maximum, and 60° on either side of the Siberian maximum. In like manner, the variation, in the 55th parallel, is 0 in the longitude of the minimum of Dip, 42° east; is easterly increasing, for the next 30° of longitude, and easterly decreasing, for the following 30°; so that the variation becomes again 0 in, or about the meridian of 102° east, which is that of the Siberian maximum of intensity.

In the sincere hope that this letter may be instrumental in promoting this highly curious and philosophical enquiry, which would be the best return I can make to M. Hansteen for his kindness in giving me so early and so full an account of the progress of his discoveries,

I remain, my dear Sir,

Very faithfully yours,

Edward Sabine.

P. S. Since I wrote the above I have substituted a needle made some years ago by Mr. Dollond for myself, for one of the two which originally belonged to M. Hansteen, and which it was my first intention to have sent to you. You will perceive, by the memoranda accompanying the needles, that No. xx, the one I have substituted, has remained perfectly steady in its magnetism for a twelvemonth past, and will probably therefore continue so. No. xi, which I received from M. Hansteen three years ago, has increased its time of making 300 vibrations from 15′ 46″.1 to 15′ 52″.7, since June, 1827, when the last published observations were made with it. Phil. Trans. 1828, Art. 1, page 14. Consequently, its magnetism has diminished, in two years, between one and two parts in one hundred. It will be prudent, however, to treat both needles as liable to further changes.

Experiments on the Force of the Earth's Magnetism. *By* Capt. Edward Sabine, R. A., Secretary of the Royal Society.

[Communicated by the Author.]

The preceding letter to Mr. Renwick, respecting M. Hansteen's recent observations on the isodynamic magnetic curves in Russia and Siberia, contains also a general notice of all existing observations on the same subject. Those of my own making, referred to in that letter, were published in 1825, in the volume containing the account of my Pendulum and other Experiments made in 1822 and 1823. That account contained the Dip of the Needle at nineteen stations, principally in the Northern Hemisphere, and the times of vibration of magnetic needles suspended horizontally, at the same stations. The times of vibration were not corrected for the effect of differences of temperature, arising from the widely different latitudes at which the observations were made, nor were they reduced to what the time would have been in infinitely small arcs. It also appeared, on comparing the times of vibration in London, before I left England and after my return, that the needles had not all preserved their magnetism unimpaired

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during the interval, but that some had undergone slight changes in that respect. A mean between the first and last times of vibration was in such cases taken as the rate in London, corresponding to that of the several foreign stations; (the purpose being to compare the force at each of the stations to the force in London). A more strict comparison would obviously have required an interpolation of the times of vibration corresponding to each station in particular; by computing the proportional part of the whole gain or loss due to the date at the several stations. And more especially it would have required that reductions should have been applied for the arcs and for the differences of temperature.

Since the first publication of those results, the subject of the magnetic intensity has increased in public interest, greatly owing to the writings and observations of M. Hansteen; and an importance has, consequently, attached to my observations greater than I ventured to anticipate at the time.

Shortly after their publication, M. de Humboldt, whose writings first excited my interest in the subject, kindly wrote me his opinion, that the minute corrections alluded to would not be, as I was disposed to consider, a refinement beyond the occasion: and M. Hansteen has since expressed the same opinion, in a review of the magnetic portion of my volume of experiments printed in Poggendorff's Annalen der Physik. I have, therefore, for some time past, viewed the correction of the results as a duty to be performed whenever leisure and convenience should enable me to execute it; and I had commenced the preliminary experiments for determining the amount of the correction for temperature for each needle, in the summer of last year. A short residence during the present summer, in the neighbourhood of the garden of the Horticultural Society at Chiswick, a spot exceedingly well calculated for such observations, has enabled me to complete them; and I cannot take a better opportunity of making the corrected results public, than at the moment when M. Hansteen's journey to Siberia is adding so greatly to our knowledge of the facts in regard to the Magnetic Intensity, and, consequently, is drawing the public attention to the subject.

I confine myself to the results obtained with the needles

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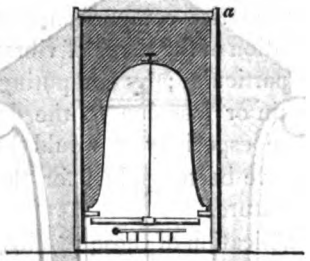
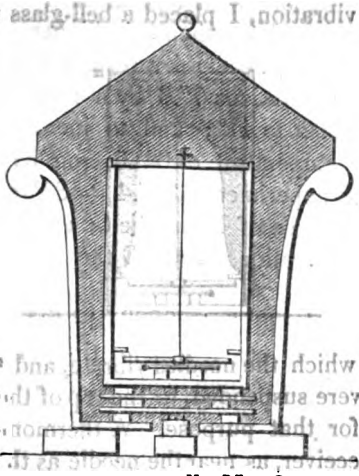
numbered 3, 4, 5, and 6, as being those from which the most satisfactory conclusions can be drawn.

In order to examine the effect of differences of temperature on the time of vibration, I placed a bell-glass receiver within-



side the box in which the needles vibrate, and fastened the silk, by which they were suspended, to the top of the receiver, which was perforated for that purpose. A thermometer was placed withinside the receiver, as near the needle as the space required for its vibration would permit. The box was then closed by the usual thick plate of glass at the top. In the apparatus thus arranged, a needle was first vibrated at the natural temperature of summer, about 65°. The glass plate was then removed, the space which is shaded in the wood-cut was filled with ice, and the plate replaced. When it appeared by the thermometer that a sufficient time had been allowed for the temperature withinside the receiver to become steady, the needle was again vibrated. On the following day, the experiment was repeated at the same hour and in the inverse order, the needle being first vibrated in the colder, and then in the warmer temperature. By this means, the difference in the time of vibration was obtained, corresponding to the usual summer heat of this country, and to more than 20° below it. To ascertain the correction for higher temperatures, the box was itself placed withinside a large garden-pot, surmounted by a garden glass. The hole in the bottom of the pot was enlarged, so that the flame of a spirit lamp entering the aperture, heated the air contained in the space between the box and the pot, and ultimately that contained within the box itself. Each of the needles were then successively vibrated, 1st, at the usual summer temperature; 2d, at temperatures

from 40° to 50° higher; and 2d, again when reduced to the summer temperature; a mean being taken between the 1st and 3d, to compare with the 2d.



With these arrangements, needle No. 3 made the time of 70 vibrations as follows:—

	August, 1828.			
In the ordinary temp.,	19′ 28″.8	Th. 64°.5		
In the cooled temp.			19′ 23″.7	Th. 40°
In the cooled temp.			19′ 22″.8	Th. 47°
In the ordinary temp.,	19′ 28″.0	Th. 64°.0		
	19′ 28″.4	Th. 64°.25	19′ 23″.25	Th. 43°.5
	July, 1829.			
In the ordinary temp.,	19′ 26″.1	Th. 67°		
In the heated temp.			19′ 35″.07	Th. 116°
In the ordinary temp.,	19′ 26″	8°		
	19′ 26′	7°.5	19′ 35″.07	Th. 116°

Whence we have, by the first experiment, an increase of 5.″15 in the time of vibration caused by an increase of 20°.75 in the temperature; and by the second mode of experiment, an increase of 8″.82 in the time caused by an increase of 48°.5 in the temperature. Then, 13″.8 − 9°.25 = increase in the time for degree of and 0″.2 ÷ 19′ 28″ 7, the [illegible] for needle Then, if T be the [illegible] needle perature t, and [illegible] the

time T, in which the same number of vibrations would have been performed at any other temperature, t', adopted as a time of comparison, $T'=T\,[1 \pm .00017\,(t-t')]$; the sign + being applicable when the observed temperature is lower than that adopted as a mean, and — when it is higher.

By the same means, needle 4 was found to lessen the time of performing 170 vibrations 7″.6 by a reduction of temperature of 21°, from 62° to 41°; and to increase it 16″.8 by an augmentation of temperature of 64°.5, from 57° to 121°.5. The sum of the two experiments 24″.4 ÷ 85°.5 = 0″.285, the increase for one degree of Fahrenheit; and 0″.285 ÷ 15′ 20″ the time of 170 vibrations = ·0003 the multiplier. For this needle, therefore,

$$T'=T\,[1 \pm \cdot0003\,(t-t')].$$

Needle 5 was found to lessen its time of performing 240 vibrations 20″.25 by a reduction of 22°, from 64°.25 to 42°.25; and to increase the time 25″.4 by an augmentation of 39°, from 68° to 107°. The sum of the two experiments 45″.65 ÷ 59°.25 = 0″.75, the increase for one degree of Fahrenheit; and 0″.75 ÷ 31′ 07″, the time of performing the 240 vibrations = ·0004, the multiplier. For this needle, therefore,

$$T' = T\,[1 \pm \cdot0004\,(t-t')].$$

Needle 6 was found to lessen its time of performing 100 vibrations 2″.78, by a reduction of 24°.5, from 67°.5 to 43°; and to increase it 4″.51 by an augmentation of 48°, from 65° to 113°. The sum of the two experiments is 7″.29 ÷ 72°.5 = 0″.1, the increase for one degree of Fahrenheit; and 0″.1 ÷ 10′ 38″, the time of performing 100 vibrations = ·00016 the multiplier. For this needle, therefore,

$$T' = T\,[1 \pm .00016\,(t-t')].$$

When observations with horizontal needles are widely extended, so as to include a considerable range of intensity, and consequently much difference in the time of performing a given number of vibrations, it is not sufficient for the just relation of the results, that the vibrations should be made on all occasions in similar arcs: it is also necessary, in such case, that the time of vibration at each station should be increased to what it would have been had the vibrations been made in infinitely

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small arcs. I have taken for this correction the usual formula, $T' = T\left[1 + \frac{\text{sin.}(A+a) - \text{sin.}(A-a)}{32\,M.\,(\text{log. sin. } A - \text{log. sin. } a)}\right]$, T being the observed time of vibration, A and a the commencing and concluding arcs, and T' the time of vibration corrected for the arc. By comparing the corrections given by this formula with the vibrations in different arcs, I have ascertained that it represents, with sufficient approximation, all the differences which are observed to take place. It was my usual custom to commence the registry of the vibration when the arc was at 30°, and to continue it till the arc had diminished to 10°. In such case $A = 30°$ and $a = 10°$, and the formula becomes $T' = T + T \cdot 0065$. In some of the repetitions which I have made with the needles in London, I have begun and concluded with smaller arcs; which exceptions to the usual custom will be expressly noticed in the cases which occur in the following pages.

I proceed next to consider what should be regarded as the time of vibration of each needle, corresponding to the periods at which observations were made with it in different parts of the world. The times of vibration in London, in 1821, 1823, and 1824, uncorrected for temperature and the arcs, are given in the account of the experiments with horizontal needles (Pendulum and other Experiments, page 481). The original record of the observations in 1821 and 1823, is still existing, and enables me to supply the necessary corrections, by furnishing a knowledge of the temperature and of the arcs. After the completion of the experiments in 1824, the needles were laid by in pairs, each pair contained in a separate box with opposite poles united, and the boxes tied up together. In this state the needles remained for four years, until 1828, when they were taken out to have the corrections for temperature determined. By collecting in one view the several observations properly corrected made with each needle in London, we may readily examine and assign the rate of vibration corresponding to the periods at which the needles were used at foreign stations. Commencing with No. 3, we have in

1821. October. In the Regent's Park. Th. 55°. Arcs 20° to 5°. Rate 165″.9 ; + 0″.14 reduction to Th. 60° ; + 0″.41 to infinitely small arcs = 166″.4.

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1829. April. Horticultural Garden, Chiswick. Th. 42°. Arcs 30° to 10°. Rate 164″.0 + 0″.5 to Th. 60° + 1″.08 for Arcs = 165″.6.

1828. March. In the Regent's Park. Th. 46°. Arcs 30° to 5°. Rate 163″.52 + 0″.56 + 0″.71 = 164″.8.

1828. August. Horticultural Garden, Chiswick. Th. 64°.5. Arcs 30° to 5°. Rate 166″.9 − 0″.12 + 0″.71 = 167″.5.

1829. July. Horticultural Garden, Chiswick. Th. 67°. Arcs 30° to 5°. Rate 166″.6 − 0″.22 + 0″.71 = 167″.1.

1821.	October.	Regent's Park	166″.4	166.0
1823.	April.	Chiswick	165″.6	
1828.	March.	Regent's Park	164″.8	166.5
1828.	August.	Chiswick	167″.5	
1829.	July.	Chiswick	167″.1	
		Mean .	166″.3	

In the case of this needle, we may consider the differences from the mean rate as accidental deviations, occasioned possibly, in part, by the observations having been made at different periods of the year, or at different hours of the day, or at different spots, and in part, also, by the errors of observation, rather than as systematic alterations in the magnetism of the needle; and we may therefore regard 166″ as the rate in London, corresponding to observations made elsewhere in the years 1822 and 1828.

I have not attempted to introduce a correction for the period of the year at which the observations were made at the different stations, because I do not feel assured that we have sufficient evidence of the amount of the change which the magnetic force undergoes, in England, at different seasons of the year, independently of the effect on the needle itself of variations of temperature: and we have no satisfactory evidence whatsoever in regard to the monthly oscillations of the force, at any of the foreign stations. With respect to the hour of the day, all the observations, at every station, were made between the hours of noon and five P. M., excepting at Jamaica and Maranham, where, from accidental circumstances, they were necessarily made between six and nine A. M. No order of succession was preserved in making the observations, the needle that first presented itself being the first used: but any irregularities arising from this cause disappear in the mean result.

With needle No. 4, we have in

1821, October, Regent's Park. Th. 55°, Arcs 20° to 2°. Rate 540″.0 + 0″.8 + 0″.9 = 541″.7.

1823, April, Chiswick. Th. 42°, Arcs 30° to 10°. Rate 532″.4 + 2″.9 + 3″.5 = 538″.8.

1828, March, Regent's Park. Th. 40°, Arcs 30° to 5°. Rate 534″.1 + 3″.2 + 2″.3 = 539″.6.

1828, September, Chiswick. Observed by Mr. David Douglas. Th. 64°.5, Arcs 30° to 5°. Rate 540″.0 — 0″.7 + 2″.3 = 541″.6.

1821, October, Regent's Park	541″.7
1823, April, Chiswick	538″.8
1828, March, Regent's Park	539″.6
1828, September, Chiswick	541″.6
Mean	540″.4

In the case of this needle also, we may regard the mean 540″.4 as the rate of vibration corresponding to all periods of the years 1822 and 1823.

With needle No. 5 we have, in

1821, August, Regent's Park, Th. 66°, Arcs 20° to 5°. Rate 726″.1 + 0 + 1″.46 = 727″.7.

1821, October, Regent's Park. Th. 55°, Arcs 20° to 5°. Rate 729″.53 + 1″.46 + 1″.83 = 732″.8.

1823, April, Chiswick. Th. 42°, Arcs 30° to 5°. Rate 745″.7 + 5″.36 + 3″.23 = 752″.3.

1828, March, Regent's Park, Th. 48°, Arcs 30° to 5°. Rate 765″.0 + 3″.67 + 3″.32 = 772″.0.

1828, September, Chiswick. Observed by Mr. David Douglas. Th. 66°, Arcs 30° to 5°. Rate 769″.4 — 1″.84 + 3″.34 = 770″.9.

1829, July, Chiswick. Th. 68°, Arcs 30° to 5°. Rate 772″.0 — 2″.47 + 3″.35 = 772″.9.

1821, August, Regent's Park	727″.7
1821, October, Regent's Park	732″.8
1823, April, Chiswick	752″.3
1828, March, Regent's Park	772″.0
1828, September, Chiswick	770″.9
1829, July, Chiswick	772″.9

Hence we perceive that this needle gradually lost a portion of its magnetism in the years 1821, 1822, and 1823, and did not become stationary until some time between 1823 and 1828, and that the loss was more rapid in 1821 than in 1822 and 1823. If, then, we distribute the total increase in the time of vibration (19″.5 in the eighteen months comprised between October 1821, and April 1823) into a monthly increase of 1″.5 in each of the first three months of that period, and of 1″

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in each of the other fifteen months, and continue an increase of 0″.5 in each of the remaining months of 1823, we shall make the best arrangement, perhaps, that circumstances will permit, for the rate of this needle in London corresponding to the observations made with it elsewhere.

With needle No. 6 we have, in

1821, August, Regent's Park. Th. 60°, Arcs 20° to 2°. Rate 621″.12 + .0 + 0″.98 = 622″.1.

1821, October, Regent's Park. Th. 56°, Arcs 30° to 10°. Rate 619″.5 + 0″.50 + 4″.02 = 624″.0.

1823, April, Chiswick. Th. 42°, Arcs 30° to 5°. Rate 634″.2 + 1″.82 + 2″.75 = 638″.8.

1828, September, Chiswick. Observed by Mr. David Douglas. Th. 67°, Arcs 30° to 5°. Rate 636″.9 — 0″.76 + 2″.76 = 638″.9.

1829, July, Chiswick. Th. 65°.5, Arcs 30° to 5°. Rate 637″.0 — 0″.56 + 2″.76 = 639″.2.

1821, August, Regent's Park	622″.1
1821, October, Regent's Park	624″.0
1823, April, Chiswick	638″.8
1828, September, Chiswick	638″.9
1829, July, Chiswick	639″.2

Hence we perceive that this needle also lost a portion of its magnetism between 1821 and 1823; but that it has been stationary from April 1823, to the present time. If, then, we distribute the total increase in the time of vibration between October 1821 and April 1823, 14″.8 in eighteen months, in the following manner, viz.—An increase of 1″.5 in each of the first five months; of 1″ in each of the next five months; and of 0″.5 in each of the remaining eight months; we may consider the time of vibration in London during those months as satisfactorily represented by the interpolated rates.

On a review of the degree of permanency with which the needles have preserved their magnetism, we perceive that three of them have undergone no alteration in that respect in the six years intervening between 1823 and 1829, during which time they have been kept in pairs, and the small boxes, each containing a pair, tied up together. This mode of keeping needles has the advantage of rendering them less liable to be injured by the accidental approach of iron or of a magnet, than if they were kept separately. Their being kept in pairs, also, probably contributes to preserve their magnetism unim-

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paired, independently of guarding them against such accidents. Two of the needles have the same time of vibration now as when they were first employed in 1821; they are both simple bars; one square at the ends, and the other pointed. A third increased its time of vibration about $\frac{1}{57}$th part in the first two years, and has remained steady since; it is cylindrical in shape, the ends not pointed. The fourth needle altered more, and was a longer time before it became steady than the others; it is a bar, with square ends, but it differs from the other needles in being made (for experiment) of iron, with extremities of hardened steel. Two such were made at the same time, and both have undergone greater changes, and have been longer in becoming stationary than needles of uniform hardness.

To exemplify the manner in which the results with the several needles are brought in comparison with each other, and in relation to a fixed term of comparison in London, I take the first station at which the needles were all employed after I quitted England in 1821, namely at Sierra Leone, March 20, 1822, Th. 80°, Arcs 30° to 10°.

Needle 3. Rate 119″.76 — 0.41 to Th. 60° + 0.78 for arc = 120″.13.
Needle 4. Rate 389″.3 — 2.34 + 2.53 = 389″.5.
Needle 5. Rate 535″.6 — 4.28 + 3.47 = 534″.8.
Needle 6. Rate 456″.7 — 1.46 + 2.98 = 458″.2.

The corresponding rates in London are, needle 3, 166″; 4, 540″.4; 5, 739″.5; 6, 631″.5. If then we take as a general mean term of comparison in London, to which the results at all stations should be referred, a needle which, freely suspended in the direction of the dip, would make its time of vibration 300″, the same needle would have its time of horizontal vibration in London, (the dip in 1822-1823 being 70°) $\sqrt{\frac{300^2}{\text{Cos. } 70^\circ}} = 512''.97$. The time of horizontal vibration of this needle at Sierra Leone would then be, according to the several needles, as follows:—

Needle 3. $166.0^2 : 120.13^2 :: 512.97^2 : x^2$; whence $x = 371.2$
Needle 4. $540.4^2 : 389.5^2 :: 512.92^2 : x^2$; whence $x = 369.7$
Needle 5. $739.5^2 : 534.8^2 :: 512.92^2 : x^2$; whence $x = 371.0$
Needle 6. $631.5^2 : 458.2^2 :: 512.92^2 : x^2$; whence $x = 372.2$
Mean 371.0

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Proceeding in this manner, with the times of vibration as given in page 481 of "Pendulum and other experiments," the arcs always between 30° to 10°, and the temperatures as severally noticed, the following results are obtained:

Madeira, January 12, 1822. In a garden near Funchal, Th. 62°:

Needle 3	440″.8
Needle 4	441″.0
Mean	440″.9

Teneriffe, January 17, 1822. On the sea-beach, near the foot of the hills east of Santa Cruz, Th. 67°:

Needle 4	434″.8
Needle 5	435″.6
Needle 6	436″.6
Mean	435″.7

Port Praya, Cape Verd Islands, January 26, 1822. In a grove of cocoa-trees, near the watering-place, Th. 73°:

Needle 4	387″.3
Needle 5	384″.7
Mean	386″.0

Bathurst, River Gambia, February 4, 1822. In a field north of the town, Th. 76°:

Needle 4	379″.3
Needle 5	379″.3
Mean	379″.3

Sierra Leone, March 20, 1822. At the foot of Tower Hill, Th. 80°:

Needle 3	371″.2
Needle 4	369″.7
Needle 5	371″.0
Needle 6	372″.2
Mean	371″.0

Island of St. Thomas, Man-of-War Bay, May 20, 1822, Th. 84°:

Needle 3	363″.3
Needle 4	364″.7
Needle 5	367″.8
Needle 6	365″.1
Mean	365″.2

Island of Ascension, July 7, 1822. In the Barrack Square, Th. 84°:

Needle 4	367″.5
Needle 5	370″.8
Mean	369″.1

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Bahia, July 26, 1822. In the suburb of Victoria, Th. 74°:

Needle 3	370″.8
Needle 4	371″.3
Needle 5	373″.0
Needle 6	373″.5
Mean	372″.2

Maranham, August 28, 1822. In a garden two miles distant from the town, Th. 80°:

Needle 4	368″.9
Needle 5	363″.6
Needle 6	368″.9
Mean	365″.5

Trinidad, September 28, 1822. In the Governor's garden at St. Ann's, Th. 86°:

Needle 3	363″.7
Needle 4	367″.2
Needle 5	363″.8
Needle 6	364″.6
Mean	364″.6

Jamaica, October 28. On the sea-beach at Port Henderson, Th. 80°:

Needle 3	354″.4
Needle 4	355″.2
Needle 5	355″.9
Needle 6	358″.2
Mean	355″.9

Island of Grand Cayman, November 11, 1822, Th. 84°:

Needle 3	359″.3
Needle 6	361″.3
Mean	359″.7

Havanna, Nov. 25, 1822. In the country south of the city, Th. 80°:

Needle 3	365″.9
Needle 4	364″.8
Needle 6	365″.9
Mean	365″.2

New York, December 27, 1822. In the grounds of the Lunatic Asylum, Th. 30°:

Needle 3	487″.7
Needle 4	488″.1
Needle 5	492″.3
Needle 6	490″.8
Mean	489″.5

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Hammerfest, June 16, 1823. On the sea-beach at Fugleness, Th. 42°:

Needle 3	614″.2
Needle 4	609″.2
Needle 5	605″.2
Needle 6	613″.6
Mean	610″.5

Spitzbergen, July 17, 1823. On the Inner Norway Island, Th. 36°:

Needle 3	723″.9
Needle 4	722″.6
Needle 5	718″.7
Needle 6	715″.5
Mean	720″.2

Greenland, August 21, 1823. On the Pendulum Islands, Th. 38°:

Needle 3	691″.2
Needle 4	690″.3
Needle 6	687″.7
Mean	689″.7

Drontheim, October 21, 1823. On the ascent of the Steinberget Hill, Th. 47°:

Needle 3	569″.4
Needle 4	577″.3
Needle 6	570″.7
Mean	572″.5

These are the times of horizontal vibration, the squares of which express the proportion which that part of the magnetic force bears at each station, which gives the compass-needle its direction in regard to the geographical meridian. To obtain from thence the total magnetic intensity, if T′ be the time of horizontal vibration, D the dip, and T″ the time of corresponding vibration in the direction of the dipping needle; $T'' = \sqrt{T'^2 \cos. D}$: and T″² will express the ratio of the total magnetic force at each station.

	T′ = 513.0″	D = 70.00°	T″ = 300.0″
London			
Madeira	440.9	61.50	303.0
Teneriffe	435.7	59.46.8	309.1
Port Praya	386.0	45.26.3	323.3
Bathurst	379.3	40.23.1	331.0
Sierra Leone	371.0	31.02.5	343.4
St. Thomas	365.2	00.06	365.2
Ascension	369.1	05.10	368.4

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	a	b	H
Bahia	372.2	04.12	371.7
Maranham	365.5	23.06.2	350.5
Trinidad	364.8	39.02.5	321.5
Jamaica	355.9	46.55.3	294.1
Grand Cayman	359.7	48.48.3	291.9
Havanna	365.2	51.55.2	286.8
New York	489.8	73.07	263.8
Hammerfest	610.5	77.13.3	287.1
Spitzbergen	720.2	81.10.5	282.1
Greenland	689.7	80.12	284.6
Drontheim	572.5	74.42	294.1

The dips which I have employed are those which I observed myself, except at Madeira, where an accident prevented my using the needle on which I could place most reliance; and I have reason to think that the needle I did use made the dip a few minutes too great; I have therefore taken it at 20′ less.

There are two other series of observations on the magnetic intensity of principal note: one made by M. de Humboldt, consisting chiefly of observations during his celebrated voyage to the equinoctial parts of the American continent, at the close of the last and beginning of the present century, twenty-four years before mine; the other made by M. Hansteen, and gentlemen who have used instruments prepared by him, in the northern parts of the old continent. These latter are either cotemporaneous or subsequent to mine.

M. de Humboldt's series includes two stations, at which I have also observed, and which are contained in the preceding list. M. Hansteen's series also includes two of the stations visited by me. It is desirable to examine how far the observations correspond. In order to do this, we must first have an accurate comparison of the magnetic intensity at the stations which have served as bases to the respective series. M. de Humboldt's observations were made relatively to the intensity at Paris; M. Hansteen's, to that at Christiana; and mine, to that at London. In the Philosophical Transactions for 1828, I have given an account of the comparison of the intensities at London and Paris, effected by means of six needles frequently interchanged between the stations; and the intensity at Christiana and London has since been compared by M. Hansteen

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and myself, by a similar process. The six needles employed between London and Paris made the ratio of the horizontal force at Paris to unity in London as follows:—

1.0722
1.0675
1.0731
1.0726
1.0709
1.0717

Whence the needle, which has served as a general term of comparison for the different stations, and is supposed to make its time of vibration in London in the direction of the dipping-needle 300″, would have its corresponding time of vibration at Paris as follows:—

1. Horizontal Vibration.	2. In the direction of the dipping-needle.
495″.2	302″.1
494″.3	302″.8
493″.0	302″.0
493″.1	302″.1
493″.5	302″.3
493″.3	302″.2
493″.4	302″.2

The dip in London, in the spring of 1827, (the period at which the comparison was made,) is considered to have been 69° 49′; and in Paris, 67° 58′.

The stations, common to M. de Humboldt's series and mine, are Teneriffe and Trinidad; (in the latter case his observations were made at Cumana, in lat. 10° 27′ N., and long. 64° 16′ W.; and mine at Port Spain, in Trinidad, in 10° 39′ N., and long. 61° 35′ W.) M. de Humboldt did not employ horizontal needles, but observed the number of vibrations which a dipping-needle, freely suspended in the magnetic direction, made in ten minutes at each station. This number was in Paris 245, at Teneriffe 238; and at Cumana 229. If, then, we suppose the relative intensity at Paris, London, Teneriffe, and Trinidad, to have been the same in 1822, as it was twenty-four years antecedently, (which supposition is probably not strictly correct, but sufficiently so for the present comparison), the needle which makes its time of vibration in London 300″, and in Paris 302″.2, would make, according to M. de Humboldt, the corresponding

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time at Teneriffe $\sqrt{\frac{302.2 \times 245'}{238^2}} = 311''.1$; and at Cumana $\sqrt{\frac{302'.2 \times 245'}{229^2}} = 323''.3$.

We have thus,—

At London		300
Paris		302.2
Teneriffe,	1799	311.1
Teneriffe,	1822	309.1
Cumana,	1799	323.3
Trinidad,	1822	321.5

The intensity at Christiana and London was compared by means of two of the same needles that were used between Paris and London, Nos. IV. and VIII. They were vibrated at Christiana by Mr. Hansteen, in January, 1828; in London, by myself, in March; in Christiana, a second time, in May; and in London, a second time, in June. Their times of vibration, reduced to a mean temperature (49°), were as follows:—

	Needle IV.		Needle VIII.	
Christiana, Jan. 10	Noon	1097″.25	Noon	886″.86
London, March 23	1 P.M.	1049″.26	Noon	850′.96
Christiana, May 1-4	Mean A.M. and P.M.	1099″.11	Mean A.M. and P.M.	890″.67
London, June 3	1 P.M.	1053″.80	2 P.M.	853″.7

Whence, by interpolation, we obtain the time of vibration in Christiana, in March, corresponding to the observations in London at that period; and in London, in May, corresponding to the observations in May, at Christiana.

Needle 4	March 23	Christiana	1098″.45	London	1049″.26
	May 2	Christiana	1099″.11	London	1051″.76
Needle 8	March 23	Christiana	889″.3	London	850″.96
	May 2	Christiana	890″.67	London	852″.46

And if we take the horizontal intensity in London as unity, in Christiana it will be as follows:—

By needle 4	Comparison in March	0.9124
	Comparison in May	0.9157
By needle 8	Comparison in March	0.9157
	Comparison in May	0.9160
	Mean	0.9147

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The dip in the spring of 1828, at Christiana, was observed by M. Hansteen to be 72° 16′.2; in London, at the same period, it was 69° 47′: whence the total magnetic intensity at Christiana, is as $\frac{0.9147 \times \text{Cos. } 69° 47'}{\text{Cos. } 72° 16'.2} = 1.0376$, to unity in London; and the needle which makes the time of vibration 300″ in London, would require 294″.47 in Christiana.

The two stations common to M. Hansteen's observations and mine, are Drontheim and Hammerfest. At Drontheim, in 1825, M. Hansteen found that his horizontal needle, which at Christiana made 300 vibrations in 816″, required 866″.77 for the same number; he also observed the dip in that year at Christiana 73° 26′; and in Drontheim, 74° 42′. The numbers 816″, and 866″.77, reduced to the direction of the dipping-needle, became 448″.30, and 445″.30; and the rate of vibration at Drontheim corresponding to 300″ in London, is $\frac{300^s \times 445^s.30}{448^s.30} = 292''.5$. In 1827, Professor Keilhau made a second comparison between Drontheim and Christiana, with a horizontal needle which had been compared at Christiana with M. Hansteen's. M. Keilhau made the relative times 816″, and 869″.7, which reduced to the direction of the dipping-needle, are 448″.30 and 446″.8; and the rate of vibration at Drontheim, relatively to 300″ in London, is 293.5. We have thus—

Sabine, in 1823 . . .	294″.1
Hansteen, in 1825 . . .	292″.5
Keilhau, in 1827 . . .	293″.5

At Hammerfest, in 1827, Professor Keilhau observed 937″.4 corresponding to 816″ at Christiana. These make the relative times in the magnetic direction, (the dip at Hammerfest being 77° 13′, page 27) 448″.30 and 440″.94; and the rate of vibration at Hammerfest, corresponding to 294″.47 in Christiana or 300″ in London, 289″.6. A previous observation at Hammerfest is recorded by M. Hansteen (Astr. Nach. No. 146) to have been made, in 1825, by some gentlemen who had undertaken the charge of one of M. Hansteen's needles in a voyage from Norway to Archangel, and who found the time of horizontal vibration 930″.8, relatively to 816″ at Christiana. This

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observation would make the time of vibration of the general comparing needle 287″.6 at Hammerfest. We have thus—

In 1825, as above . . .	287″.6
In 1827, M. Keilhau . . .	289″.6
In 1823, Capt. Sabine . .	287″.1

These results approximate perhaps as nearly as can be expected in the high latitudes, where the small, secular, and (probably) diurnal variations in the dip, occasion much greater alterations in the horizontal intensity than take place in the lower latitudes.

A remarkable Phenomenon of Sound, and of the conveyance of minutely divided Matter, during the Eruption of Mount Souffre in 1812.

From many observations, I have found that thunder can rarely be heard at a greater distance than twenty miles. I have also had occasion to notice, that the eight o'clock gun of Georgetown, in Demerara, is often heard at Cape Batave on the west coast of Essequibo, a distance of about forty miles. The distances, however, were almost incredible to which the concussive motions arising from the tremendous explosions which occurred during the eruption of Mount Souffre were propagated through the atmosphere. These eruptions were heard at the distance of 600 or 700 miles, namely as far as Cayenne, at Varinas, and, it is said, at Santa Fé.

The whole of the coast, and most of the West India islands, were alarmed by loud reports seemingly of great guns, which were universally supposed to be caused by a sea engagement; and the ships of war among the islands and on the coast of Guiana sailed out to reconnoitre, as they supposed, an enemy, who, however, was nowhere to be found.

These reports were very loud at Pomeroon, where I heard them like the firing of cannon, incessantly for nearly two hours. They excited so much consternation in the town, from the idea of an engagement on land, as to put the troops in motion.

That sound should be conveyed, or, in other words, that a

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vibrating motion of the air should have been propagated to such a distance as we find in the case before us, exceeds, I believe, every instance of the kind, and has not a parallel in history.

Soon after these occurrences, we received information from Barbadoes, that the whole of that island had been visited, on the 1st of May, not merely by a nocturnal darkness, but by a complete and total darkness, which continued from about the hour of eight, A.M., till twelve at noon, and was attended by a continued shower or fall of fine sand, which completely covered the surface of the island. This wonderful phenomenon, as might well be expected, excited the astonishment of all, and threw the inhabitants into the utmost consternation. The cause being unknown, they considered it an express visitation or warning from the Deity. It was also stated, but in a vague and indefinite manner, that, in the night preceding the phenomenon, some reports were heard like cannon, and flashes of lightning were seen.

The next arrival brought intelligence that, at two or three o'clock in the morning of the 1st of May, the mountain Souffre, in the island of St. Vincents, had burst forth with the most tremendous explosion, surpassing that of the heaviest artillery, throwing up immense volumes of thick dense smoke, and livid flames, ejecting red-hot rocks of enormous weight to a prodigious elevation in the air, while rivers, as it were, of ignited minerals rolled down the sides of the mountain. The whole surface of the island had been covered with volcanic ashes, sand, and vitrified earths, the more ponderous substances naturally falling more adjacent to the mountain. Provisions and all vegetation had been destroyed, and the inhabitants reduced to a state of starvation.

The cinders were, on this occasion, thrown around to vast distances. I have a sample which was taken on the 13th from the deck of a vessel 150 miles to the windward of Barbadoes. This may seem incredible; but it is well known that the same phenomenon was witnessed on board many other vessels. History furnishes only an instance of the cinders of Mount Etna having been thrown upon the African coast.

It must have been owing to a continuous and inconceivable

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explosive force of the volcano, or to shocks repeated in quick succession, so as completely to overcome the pressure of the atmosphere, to project its volume of cinders and heated vapours to the higher regions thereof, and to cause their reverberating around, that the sound and ashes could be conveyed to such incredible distances.

I have recently observed a notice*, which is interesting, but requires to be reconsidered, as the phenomenon alluded to is undoubtedly referred to a wrong cause. It is brief, and I here transcribe it.

"Distance to which minutely divided matter may be carried by wind.—On the morning of the 19th of January last, Mr. Forbes, on board the Clyde East Indiaman, bound to London, in lat. 10° 40′ N., and long. 27° 41′ W., and about 600 miles from the coast of Africa, was surprised to find the sails covered with a brownish sand, the particles of which, being examined by a microscope, appeared extremely minute. At two, P.M., the same day, some of the sails being unbent, clouds of dust escaped from them on their flapping against the masts. During the night, the wind had blown fresh N.E. by E., and the nearest land to windward was that of the African coast lying between Cape de Verd and the river Gambia. May not the seeds of many plants, found in remote and newly-formed islands, have been thus conveyed?"

It would be surprising, indeed, were it ascertained that this sand had been conveyed from the coast of Africa by the common course of the wind. The supposition, however, is too absurd to be admitted. It was undoubtedly volcanic sand from some eruptions in the region of the Cape de Verd Islands, the more southerly of which, as St. Jago and Fogo or Fuego, bore N.E. from the ship, the distance being about 300 miles instead of 600. If inquiry were made, it would probably be found that some awful explosion had occurred at the volcanic island of Fogo or its vicinity on the night of the 18th of January. I should suppose this might be ascertained by inquiry of the masters of such vessels as were then lying

* In the New Monthly Magazine, for February, 1827.

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at St. Jago or at Brava, which is contiguous to Fogo, and frequented for its wines—the same I presume as are called Cape wines, although said to come from the Cape of Good Hope.

JOHN HANCOCK.

On Classifications of Rocks.—By J. Mac Culloch, M.D., F.R.S., &c.

IN every branch of natural history it has been a principal object to adopt such arrangements of the bodies of which it treats, as should facilitate their study. The most purely artificial classification may thus be useful; but the naturalist of higher views attempts to discover the order which Nature herself has instituted, and thus, if possible, to combine, with utility, the history of those analogies which form the basis of all science.

Such is the wide and various range of geology, and so important are the great relations and analogies which it involves, compared to those which regard only its minuter details, that it has been the object of almost all the cultivators of this science to found, at least, the chief features of its classification on certain leading relations. As the essential circumstances involved in this question do not admit of being examined in this paper, the reader must be supposed to understand the following statements, without entering into much minuteness of detail.

Though presenting to the eye infinite varieties of aspect, the real and definable differences of rocks are very limited. Hence, no great number of names has been required for distinguishing them. It is also found, that in a single mass or stratum of one rock, bearing one fixed and steady relation to its associates in nature, the aspect, the proportions, the mode of mixture, and the nature of the ingredients, are subject to variations; and hence a still narrower limitation of the number of names, which a superficial consideration might have been inclined to apply to them, has been found necessary.

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Thus, the different varieties of granite, of gneiss, of micaceous schist, or of other rocks, have been approximated under certain leading mineral characteristics, so as to found mineralogical families; each of which, however various its members, is distinguished by one general title. The term genus has, by some, been applied to these associations; and that of species, by others; but with no great propriety, as they are not amenable to the rules which regulate the forms and differences of organized beings, to which alone it is possible to apply these distinctions usefully. On such a basis may be formed a mineralogical arrangement, or classification of rocks; and on it, arrangements of this nature have actually been attempted.

Now, it is further found that, in nature, there are certain rocks, or families, which possess either a constant or a prevailing position; or that, where many occur together, some one is always the lowest, and some other the highest. If this arrangement were as perfect as it has been imagined, there would be a fixed numerical order of succession; or every rock might be indicated as well by a number as by a name. It is now well known that this is not the fact; though there is a prevailing order, which may be rendered of use in geological science. It is also found that there are certain prevailing associations among rocks, or families, in nature; and that, in each of these, whatever differences may occur in different places, there are some general rules to which we can always safely refer. Lastly, it is also observed, that, of these different rocks, there are some which are always distinguished by their stratified disposition, while others are as invariably found in shapeless and irregular masses. Such facts present a basis for a geological classification of rocks, or for one which, comparatively disregarding their mineral characters, attempts to arrange them as integrant parts of the structure of the earth, or of the great system of nature.

It must now be observed, that certain mineral characters prevail, to the exclusion, more or less complete, of others, in each of those families of rock which are thus distinguished by their geological positions and relations. If these characters, in rocks, were steady and perfect, a mineralogical arrangement might itself be rendered perfect for geological purposes; if the

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association of such mineral characters with certain geological relations were constant, a geological arrangement would serve all the purposes of a mineralogical classification, or the latter would equally perform the functions of both; but to render the arrangement absolute and unexceptionable, the geological order of nature should itself be also constant.

The mineralogical method of classifying rocks is, however, not only imperfect, even in its own internal mechanism, but is at perpetual variance with the geological one, as I have fully shown in that work which treats of the Classification and Description of Rocks. It is therefore not only useless, even for its own declared objects, but is pernicious when adopted for geological purposes; for which cause it was there rejected.

But as I have there also shown that a constant general peculiarity of mineral character is attached to each of the geological divisions of rocks, a geological arrangement, imperfect as it is, and as it probably ever will be, is not only as valuable as a mineral one for the mere distinction of rock specimens, but, in some cases, preferable; while it is decidedly superior as it relates to the investigation and study of nature.

The very basis of the study of geology is the knowledge of rocks; but that knowledge is of little value for any other purposes but the study of geology. The mere mineralogist may be permitted to adopt a mineralogical arrangement; he ought indeed to do so; because, to him, rocks are but the repositories of minerals.

The classifications which I am about to describe have either been intended for the purpose of facilitating the study of geology, or they have been considered as a branch of the science, a declaration of the order and arrangements of nature. Thus, they may be imagined to be either artificial or natural.

A principle of arrangement, some kind of logic, good or bad, seems to be an inherent propensity in the human mind; but, while the sound reasoner adapts his logic to nature, the proposition is more commonly reversed, and nature is tortured into forms, against which she rebels. But as it is one of the qualities of a system to assimilate every thing to itself, an artificial arrangement soon comes to be considered as a natural one; and when even its inventor learns to see only through

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the false medium which he has constructed, it is not wonderful if those who follow him cease to inquire, and receive as proved, that which is mere matter of hypothesis. I hope to show that the classification of rocks which have been esteemed natural, are, on the contrary, artificial. They must not, therefore, be allowed to enter as constituents into the science of geology; and the only inquiry will be, how far they are useful in facilitating its study.

An imperfect and ill-founded observation of Lehman gave rise to the first notion of separating rocks into two classes. That observation, as far as his researches and information extended, appeared to have for its basis a real order of things; but, while the general principle has been preserved, the increase of observations has accumulated so many exceptionable facts, as to render it impossible any longer to reconcile to it the state of our knowledge or the order of nature. It was conceived that rocks could be easily distinguished into primary and secondary; the first being chiefly characterized by high, and the last by low angles of elevation; or, as more decidedly stated, rocks were distinguished by their vertical and by their horizontal positions. Hence the primitive and secondary classes were established.

Other geologists, improving on this system, have pointed out the place where these two classes meet, or have attempted to assign their common boundary; and thus the arrangement into two classes has been made more useful, if not more natural. But as this distinction has been found to be attended with some difficulty, a still more minute division has been attempted, and rocks have been arranged in three classes; a division to which the name *Transition* has been applied as an adjective term, having been introduced between the primitive and secondary. This arrangement, promulgated by Werner, has been adopted by many persons, and has been esteemed strictly natural. This classification also has been connected with a system of cosmogony, even more decidedly than that which we must attribute to Lehman; but it is not worth an inquiry which of the two was the origin of the other, whether the cosmogony or the classification preceded. It will presently appear that it is not only as defective as the more simple

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arrangement, but that it is not founded, as far as any evidence yet goes, on the real order of nature, while it is of far less utility even as an artificial arrangement.

To examine, in the first place, the simple classification, or that into primitive and secondary.

The primitive rocks are distinguished by the following circumstances. Whatever geographical places they may occupy in nature, they are the lowest in geological position; or else those rocks which steadily maintain the lowest places in the order of relative superposition, up to a certain point, are considered as primitive. It is further stated, as characteristic of this class, that the strata are always elevated at high angles, and that they follow in consecutive parallel order. It is also said that their nature or texture is chemical, or that they bear no marks of mechanical origin; and, lastly, that they do not contain organic remains. This class, thus determined, is found to comprise a certain number of rocks, of which the mineral characters are, to a great degree, peculiar and sufficiently constant; and thus also, reversely, these mineral compounds are considered, wherever they may be found, as primitive rocks.

The secondary class is, of course, the receptacle of all those not included in the other. It is supposed that these are characterized by prevailing low angles of elevation, by the general, or frequent, or necessary mechanical nature of their texture or nature, and by their containing organic remains. It is also supposed that their order is parallel and consecutive among each other, but that this order is not parallel to that of the substances in the primitive class. Hence, therefore, the boundary between the two is placed at that point where the change of order takes place; or the reverse position of approximate strata indicates that the lowest of these is the last, or uppermost, of the primitive, and that the other is the first, or lowermost, of the secondary class. If, in certain cases, it happens that this reverse position does not exist, then the boundary is determined by the mineral characters of the approximate rocks; it being observed that a conglomerate or sandstone is the lowest rock, whenever the series of the secondary strata is complete. If, again, even that rock should be absent, then

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another stratum which, from previous observations, is known to lie above the conglomerate, is assumed at that point as the lowest of the secondary class. Such is a view of the arrangement into primitive and secondary; giving it all the advantages that have been derived from observations far more recent than those of its original inventors. The limitation to its truth and utility will be examined when the nature of Werner's attempted improvement on it has been stated.

In this arrangement the division is threefold, or into primitive, *transition*, and *floetz*; the last being a term for which secondary may be substituted. The transition class, which alone requires notice here, is supposed to be distinguished from the primitive by its containing rocks, of which the nature or texture is partly mechanical and partly chemical; and further, by the organic remains which are found in its members. Hypothetically, it is also distinguished by a presumed difference in origin; a matter which cannot be examined here for want of space, and which must be referred to an examination of geological theories, and to some possible future opportunity.

Thus this class is, as the name expresses, a collection of rocks, not only formed at a period intermediate between the formation of the primitive and the secondary, but necessarily occupying an intermediate place. It is proper to examine how far this subdivision is either natural or useful, as it involves a set of objections distinct from those which apply alike to both the systems.

It is not natural, for the following reasons. In the primitive class, even of Werner, many rocks, such as micaceous schist and quartz rock, sometimes contain fragments, and are therefore partly of mechanical origin. Conglomerates also occur in this class, as among the limestones and serpentines; and it cannot here be necessary to adduce examples of these facts. If required, the reader may consult the Classification of Rocks for them. In the transition class, again, limestones and greenstones, or basalts, of a purely chemical nature, are frequent; and hence the distinction attempted to be founded on these circumstances has no existence. Organic remains are not necessarily found in the transition rocks, and thus far

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that character is defective. I have shown further, that these have been found under gneiss; whence, according to the arrangement, that gneiss ought to be a transition rock; and thus every rock found above any one that is discovered to possess such an organized body, must necessarily fall into the same class. The consequences are here sufficiently obvious; and it is equally plain that there are no characters, either singly taken or combined, by which transition rocks can be distinguished from the primitive. Neither is there any boundary by which the transition class can be separated from the first. None has been assigned; and it is evident, from the last remark, that whatever boundary may be assumed, it must be removed as often as any organic substances, or even any imbedded fragment of a rock, shall be found in a position, or in a rock, inferior to that which has last been fixed on as its lowest member. Lastly, the transition rocks are frequently absent altogether; a fact which, when the primitive and secondary are present, implies a contradiction in terms. This class, therefore, as a natural division, has every possible defect; as it is not always present, does not form a transition, has no permanent or certain characters, and has no assignable boundary.

Such at least is the judgment which must be pronounced upon it, as far as our knowledge of the early rocks yet goes. It is not impossible, as I formerly suggested, that there may be a division of rocks intermediate between the primary and secondary, and it is possible that its characters and boundaries are assignable. It is even possible that some of those which have been named as *transition* rocks, may belong to such a division; but it is very certain that, at present, we have no definite notions of it, and cannot pronounce even on its existence, much less on its characters and boundaries. As an artificial arrangement, it serves no useful purpose, as it does not facilitate the examination or classification of rocks. On the contrary, it is pernicious, as it leads to a vicious kind of reasoning in a circle; certain rocks being first called by the name of transition, and then used to determine the class. To a certain extent, it is true, the same incorrect mode of reasoning may be applied to the primitive and secondary

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classes, whenever the mere character of a specimen is used to determine either; but, in this case, we can always prevent any error by having recourse to their actual positions in the great series of rocks, which are, comparatively, of a very decided nature.

I have thus examined the objections to this class, taking it only on the general broad view which has been given of it. But it would not be doing the geological student that justice which he is entitled to claim from a writer who undertakes to tell him the truth as far as it can be discovered, if I did not state them more particularly. Its ingredients, or members, the reader may perceive, have never been named or defined, so that it answers any temporary purpose that may be wished; adopting or renouncing at pleasure, and thus, like other visionary things, remaining unassailable. That which is no where is nothing; the place of that which may be any where can never be known.

If, with some writers, it comprises the later schists and limestones of the primary class which contain shells, it is still undistinguishable, even in this arrangement; since, according to a celebrated writer on the Pyrenees, the transition rocks there alternate with micaceous schist, which is admitted to be primary. But it is easy to show that it involves both the primary and the secondary classes, that the only easily assignable rocks in it belong to the latter, and that it has probably arisen from an entire misconception respecting these. It would be easy to show, had I here space for that purpose, that the inventor of this system had mistaken the upper red sandstone for the lower; and hence certain strata inferior to that, but still in the secondary division, became members of his transition class. Proceeding then to assume a wrong criterion, namely, the presence of organic remains and of fragments, one or both, some members of the primary class possessing these characters, also fell into it. The effect of this invention, in the details, has therefore been to confound and intermix the two chief classes, in addition to all the objections already stated. It is easy to comprehend the inextricable confusion which it has introduced into geological writings; a confusion indeed so great, as to render nearly unintelligible

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many works otherwise valuable; since nothing but minute and careful details of the connexions of the rocks described in them, can enable us to translate them into a meaning. Among the works of those who use names instead of descriptions, it would be quite fruitless to seek for one; and it is perhaps the safest practice to avoid those in which the term transition occupies this prominent place.

I may now venture to examine the objections which apply to the ancient division into primitive and secondary, as well as those which may be made to that method of arrangement, under certain modifications. It will be seen that the latter is free from some of those which apply to the former. I need not dwell on the objections that first meet the view in the ancient arrangement, namely, the confounding of granite and trap with the stratified rocks. With respect to the stratified, it is by no means true that, in the primitive class, they are necessarily placed at high angles. On the contrary, they are found at every angle, even down to the horizontal, though it may be admitted that angles exceeding 15° are more common than small ones. In the same way, so far are the secondary rocks from being necessarily horizontal, or placed at low angles, that they present the utmost variety in this respect. The first member of this class, the red sandstone, is frequently elevated to very high angles; the coal strata are noted for their irregularity, and even chalk has been found in a vertical position.

It is further supposed, in this ancient division into primitive and secondary, that a consecutive order, or common parallelism, exists in each class; and that this order in the one is not consecutive to that of the other, has been more than once noticed. It is thus supposed that a general disturbance or elevation of the primitive strata took place before the deposition of the secondary, and that these also were all deposited under another period of repose. Now it is very well known that, in the secondary class, there are abundant proofs of changes of position, and that there are in it breaches of the parallel consecutive order.

The conclusion to be drawn from these objections is obvious. The arrangement, even into two classes, ought to be considered an artificial, not a natural one; and those characters which

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have been supposed to distinguish the secondary class are defective. But, to pass over other objections, it is also deficient even as an artificial arrangement, and in points that would admit of a very easy remedy, by a slight alteration. Both classes, and the secondary more particularly, comprise rocks that ought to be separated, and which might be so without difficulty; such as the unstratified substances, the partial deposits which have, by some, been called tertiary, the alluvial substances, and the volcanic rocks. The remedy for all these defects is easily found, by merely substituting certain modifications, which permit the general division into two classes to remain as it did before.

In this amended classification, the term primary is substituted for primitive, because it implies no theoretical considerations respecting the origin of rocks. The classes, otherwise, remain the same, except that they are first subdivided into unstratified and stratified rocks; a distinction, of which the propriety, and even the necessity, must be apparent to those who have read the various papers and works which I have on different occasions written, and the arguments from which I cannot here occupy room in re-stating.

The primary class, therefore, in this arrangement, as far as the stratified rocks are concerned, is defined as it was before.

All those unstratified rocks are also placed in this class, which are found to lie beneath the whole or any one of these strata, or even merely beneath the secondary, provided they do not, in any case, also lie above any one secondary stratum. They will not be taken out of this class though they should be found intruding, in the form of veins, into the primary strata; unless these veins are connected with secondary masses, or may be inferred to have proceeded from such; but they would no longer appertain to it should they continuously intrude among the secondary rocks.

In practice the distinction thus becomes geologically easy, whenever the two classes are found together. But if the primary class is found alone, as there is then no opportunity of such a comparison, it is necessary to be guided by the mineral characters of the rocks, and by such other circumstances as experience has shown to be peculiar to them. This defect,

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such as it is, must inevitably occur, whatever arrangement may be adopted; as it is not usual, nor even very common, for both classes to be found in contact, or at one point.

The secondary class commences as in the ancient arrangement, where the primary ends; but as that proceeds downwards, this extends upwards. A conglomerate and sandstone frequently, but not necessarily red, is, as already observed, its lowest possible boundary. But as this may be absurd, it is not a necessary one; and that must then be sought in the lowest stratum or rock, which is known to be, on ordinary occasions, superior to it, and of which the mineral characters, as in the case of the primary strata, are known. No rules for the boundary of this class must be drawn from the nature of the primary rock on which its lowest member reposes; because this may be the lowest as well as the highest of that class. Such also are the occasional deficiencies in the secondary class, that its upper members may rest on the primary, and even on the lowest of these. Thus the extremes of both classes might meet; an instance approaching to which has been elsewhere quoted by me as occurring in Sutherland. With respect to the unstratified substances, every such rock is considered as belonging to it which is not included in the primary division. But those of this class differ in one important point from the corresponding ones in the other; as they may, and do, send veins through the primary rocks, without forfeiting their class. It needs only be added, that their masses are generally superior to the strata with which they are associated.

Such was the arrangement adopted in the work on Rocks, for the purpose of describing their characters; and as such, it was suffered to remain, in other points, irregular and incomplete. As the rocks of the tertiary strata, for example, agreed in character with corresponding ones in the secondary class, no attempt at a third distinction was made. In the same way as jasper and some other substances occur in both classes, it was held unnecessary to repeal them in both, where one discussion would serve, so that they were placed in an appendix, as being merely accidental modifications of some members in both classes. Such an arrangement, it is evident, made no pretensions to a classification, in the proper sense of the term. With

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equal neglect of geological classification, the remaining substances, comprising volcanic rocks, coal, alluvia, and other matters, were placed in another appendix, because it would have been impossible to have followed a geological order in the descriptions of these, without the greatest inconvenience.

These deficiencies, however, being palpable and acknowledged, were of little moment; particularly as the arrangement was not intended to form part of a system of geological science. The most serious defect was one, which, I fear, is still irremediable. It is that which relates to the overlying rocks; a division, the very name of which is objectionable. There can be no doubt that some of the porphyries and claystones are truly primary; but the difficulty was then, as it is now, to determine which were such, and to find a mode of distinguishing them from secondary ones. Thus the whole were placed in one family, and in the secondary class; a plan which had no other merit but its convenience for the purpose of describing the several kinds with the least repetition or confusion.

There are some practical defects, I must now add, in a classification of rocks, were it even a perfect one, inherent in the very nature of the subject, and consequently irremediable. One of the most obvious of these, is the repetition of the same substance in different classes: of which limestone, jasper, siliceous schist, and others, are examples. Custom has led us to treat of the limestones of the two classes separately; yet to attempt to do the same for the other substances would not now be tolerated; besides which, there is perhaps not enough to be said respecting them, to justify such a division. In the same manner limestones, sandstones, and other substances, occur in the secondary class, and also in what may be called a tertiary one. The former, indeed, is even an alluvial rock, in the shape of travertino and coral sandstone; so that it occurs in four distinct parts of the system. But I need not enumerate more of these defects and difficulties. They cannot be remedied; but we must recollect that they are in a great measure artificial evils, and of our own creating. They have arisen from one of those abuses of logic, which attempts to reduce every thing to one system; from a desire to carry into a

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department of nature, where it is not applicable, that kind of arrangement which had been found successful in others.

Though there is no prospect, therefore, of producing a classification of rocks, either natural or artificial, which can be followed in a systematical and descriptive work, there is no reason why we should not attempt to arrange them in some order. Such a classification, could it be perfected, would still have its uses in a scientific view, as it would represent the order of nature, or at least declare the connections, analogies, or relations, of rocks, in a compendious and convenient manner. It would also be of use for the mere purposes of description and arrangement; because it would be a point from which to depart or deviate, without the risk of straying too far or falling into confusion; standing as a beacon to indicate such changes as might, from time to time, be made in it. Even if highly imperfect, it would still be useful; because, by placing the imperfections and difficulties in an obvious light, and by showing those that actually exist, it would lay the first step towards a better system.

I am very far indeed from presuming that I have discovered an unexceptionable classification; but if no one will attempt an imperfect one, we are very little likely to succeed in finding one that shall answer the desired ends. It is also true, in every thing, that what an inventor has been unable to accomplish, is often done in an instant by those who might never have made the original suggestion.

It seems to me that something like a natural classification may be made on the basis of the materials collected for that System of Geology which I have long since prepared for the press, which has remained these many years dormant in its MS., and may possibly thus remain for ever. The reasons for distinguishing the unstratified from the stratified rocks, in both classes, must long since have been obvious. Those for dividing the stratified rocks of the ordinary secondary class into three, are, in my own estimation, satisfactory even as a question of geological theory; but being here compelled to crowd what I can into the narrowest possible limits, I dare not attempt to state them.

I shall only further premise respecting this classification,

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which is here given in a tabular form, that it coincides so well with that which has been already used for the practical purposes already mentioned, as to admit of their both standing together. Very few modifications were, in fact, required to permit the same enumeration to serve for both; and thus two modes of division have been applied to the same list. Thus, one of them represents what may hereafter be rendered a natural classification in reality, as it is, now, but an attempt at one; while the other is an artificial one, which may safely be adopted, at least as such, subject always to future amendment. I have merely numbered the new classes for the present, from an aversion to introducing new terms. If geologists shall approve of this plan under the present or some modified form, it is possible they may not feel the same dislike to the Greek compounds, of which I have merely suggested the first. I know not very well, if there must be a name, what better expedient could be devised.

ARTIFICIAL SYSTEM.		NATURAL SYSTEM.
PRIMARY CLASS.		CLASS 1. Protolith? &c.
STRATIFIED.		
Gneiss.		
†Micaceous schist.		
Chlorite schist.		
Talcose schist, partial.		
Hornblende schist.		
Actinolite schist, partial.		
*Quartz rock.		
*Red sandstone.		
†Argillaceous schist, fine clay slate and graywake.		
Diallage rock.		
Serpentine, ambiguous.		
‡Limestone.		
Compact felspar, partial.		
Jaspar modified from *	Where granite or trap are present, partial.	
Siliceous schist, modified from †		
Chert, modified from ‡ when argillaceous.		
UNSTRATIFIED.		CLASS 2.
Granite, from the ordinary appearance to that of greenstone and basalt.		
C. felspar porphyry, where decidedly limited to the primary strata	Often doubtful.	
Clinkstone and claystone, in the same cases		
Serpentine, when solely connected with primary strata, or granite.		

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	ARTIFICIAL SYSTEM.	NATURAL SYSTEM.
	SECONDARY CLASS.	CLASS 3.
	STRATIFIED.	
Series below coal.	*Sandstone. Conglomerate and fine. Old red, of various colours and qualities.	
	Limestone. Mountain limestone of English. Transition of some.	
Coal series.	Sandstone, various.	CLASS 4.
	†Shale } ditto.	
	*Clay }	
	Limestone.	
	Coal.	
Series up to chalk or marine clay.	Limestone—Magnesian, first above the coal series.	CLASS 5.
	,, ‡Argillaceous. Lias. Muschelkalk, &c.	
	,, Compact, various; generally organic.	
	,, Oolithic.	
	,, Chalk.	
	*Sandstone—Red marl. Red sandstone, containing salt and gypsum.	
	,, White, various, and sand. Quadersandstein, &c.	
	,, Green, and sand }	
	,, Ferruginous, and sand }	
	†Shale } Various.	
	*Clay }	
	Jasper, modified from * } Partial, only where trap-rocks are present.	
	Siliceous schist, modified from † }	
	Chert, modified from ‡ }	
Salt and gypsum considered as minerals in this class.		
	UNSTRATIFIED.	CLASS 6.
	Serpentine, as connected with trap chiefly.	
	Pitchstone, comprising pearlstone. Known in veins only.	
Overlying Rocks.	Trap—Indurated clay; base of the amygdaloids.	
	,, Claystone.	
	,, Indurated claystone; comprising some basalts.	
	,, Clinkstone.	
	,, Compact felspar.	
	,, Basalt; of hornblende only.	
	,, Greenstone.	
	,, Syenite. Sometimes emulating granite.	
	,, Augit rock.	
	,, Hypersthene rock.	
	,, Porphyries; with various bases.	
	,, Amygdaloids.	
	TERTIARY CLASS.	CLASS 7.
Paris and other places.	Limestone }	
	Sandstone }	
	Chert. Millstone } Uncertain successions of marine	
	Shale } ,, and fresh water strata; or all	
	Clay } fresh water.	
	Marl }	
	Gypsum }	

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ARTIFICIAL SYSTEM.	NATURAL SYSTEM.
ALLUVIAL CLASS.	CLASS 8.
Marine, elevated; loose. Italy, containing marine remains.	
„ „ indurated. Marl of Italy.	
Coral. Islands of Pacific, &c.	
Terrestrial, loose, of very various materials.	CLASS 9.
„ general, of diluvian origin, or else elevated and marine.	
„ local, from transportation; and from decomposition.	
„ soil.	
„ solid, Travertino, of Italy.	
„ „ West Indian and Coral sandstones. Messina and others.	
„ „ Marl of lakes.	
„ „ Peat.	
VOLCANIC CLASS.	CLASS 10.
Lava, Scoria or pumice, Tufa } Presenting, under these general terms, analogies to many of the trap-rocks.	

I know not that I have much to remark on the preceding table, which the reader who is versed in geology may not remark for himself.

The repetitions of the same rocks in the different classes became a matter of course in a classification of this nature.

I have attempted to distinguish some porphyries as exclusively belonging to the primary class; as is unquestionably the fact.

In the 5th class, I have given only the principal varieties of the different rocks. To form a classification on the basis of the English strata, would be to prejudge a question of which we know not as yet enough, and to commit again an error from which geology has already severely suffered.

To distinguish more minutely in the secondary or tertiary classes, would be to introduce a new and insufferable arrangement; because the same principle must also extend to the primary, giving us three or four kinds of gneiss, of quartz rock, and so on. Important as particular strata may seem in an English or any other series, it must be remembered that they are still but varieties, in the general system.

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I have made no attempt to place the strata in the order of nature, for sufficient reasons. In the first class, there is no order in nature; there is none in the coal series. In the fifth class, the order of England is an order among varieties which are excluded by the arrangement. In the seventh, the same is true. I could gladly have extended the minute commentaries on some of the names; but that was inconsistent with the tabular form; and as I have now exceeded the prescribed bounds of this paper, I cannot venture to add them in any manner.

Remarks on the Worari and Sirvatan.

Centuries have now elapsed since this dreaded weapon, which takes away life like a magic wand, without causing the slightest pang, became known to Europeans, in its effects at least. It is strange, therefore, that the subject should still remain involved in such profound mystery, with regard to the poison, the mavacuri plant, which affords it, and that instrument, the sirvatan or blow-pipe, through which it is propelled upon the victim.

The question, what plant affords the worari poison, involves, I presume, one of the most interesting inquiries in the whole department of natural history at the present day, and deserves from us a particular and attentive investigation.

Having examined the Mandavacs, Francisco and Domingo, two intelligent Indians, who were born and bred on the spot, of the tribe most famed for producing the most active worari, and who lived in the vicinity of the mountains which produce both the deadly poison and the instrument of its conveyance, I have received from them separately a most correct and satisfactory account of this affair.

These Indians stated, that, both for the mavacuri and sarsa, they go up the Siapo and contiguous streams, or about the mountains of Unturan and of Achivucary, as observed by Humboldt*.

* They persist that there is no sarsa in Cassiquiari nor in the Rio Negro.

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They could give, however, no information respecting the flowers; but they know the plant well, and call it mavacuri; and they state, that it is of the gourd kind, or one of the cucurbitaceæ, of the size of a large orange, round, and having a hard shell or pericarp, which is used at times to contain the poison.

The mahwy, they say, is the plant of which they make the blow-pipe for projecting the arrow.

This plant, according to their representation, has large roundish leaves, is jointed, and has slight partitions, like those of the trumpet-tree, which they punch and clear away with long sticks of hard wood, fitted for the purpose. On further conversation with Domingo, it appears to be a species of palm, as, in respect to the texture, leaf and seed, he compares the different parts to the eta and camawari.

On showing him the small pigmy palm growing on the sands of Essequibo, he said it was the wahwy; exactly in respect to the stem; but not the leaf, as that is bifid, and that it was similarly jointed.

The lining tube is of the same material, a junior or smaller plant of the same kind.

In regard to the manufacture of the poison, Domingo and Francisco say, that they, in general, add nothing, though some, to thicken it, add the bark. They merely peel or scrape off the bark, and bruise it well in a mortar. The mass is then put into a funnel or cartocho made with wild plantain leaves, and having a little cotton at the bottom to strain it; plenty of cold water is poured over it; and they proceed in the same manner as in drawing the lixivium of ashes. This infusion is put into an earthen pot (that which is here called a buck-pot), and boiled down to a proper consistence.

This was related circumstantially by Domingo and Francisco, separately.—They had no idea of the addition of other substances (ants, &c.), serving, in reality, only to dilute, and render the poison less active, as prescribed by the Indians living near our settlement, all of which are but inventions like those of the charlatans of Europe to throw mystery over the affair, and enhance the value of the art. It is very surprising that men of good sense, like Mr. Waterton and Mr. Hillhouse,

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who, as I should suppose, have had opportunities of better information, should have the credulity to notice or respect such fictions.

The following extract from a letter of Mr. J. Forsyth will throw further light on the subject:—

"I received your letter of the 30th ult. requesting a specimen of the worari vine. I am sorry it is not at present in flower; but I send you a small branch of it, and two other vines, called worarybally and courampoey, which the Indians use as auxiliaries to strengthen the former. You will also receive two small roots of the worari vine, which will grow if immediately planted; it will require a great proportion of sand mixed with the earth it is planted in, as it is found growing on sand hills.

"The mode of preparing the poison is as follows:—The inner bark or rind of the root (for it is the root only that is used) is scraped off into some vessel. The worarybally root undergoes the same process; but it is the vine itself of the courampoey that is used. To these, mixed together and well boiled down with some water, the Indians add some peppers, and further boil the whole mass to a thick syrup.

"This account of the process, I have had from the Indians; but they are to bring some of these roots, &c., and make the poison in my presence. I shall, therefore, have it in my power, I hope, hereafter, to give you a more accurate description of this process."

If such a thing does in reality exist in nature as a direct sedative, in the strictest sense of the term, I should imagine it to be this extraordinary vegetable extract. Its operation on the animal frame is most mysterious. It extinguishes the vital spark without a pang or a struggle, if prepared without any other substance being added, for the most efficient poison is prepared from the worari vine alone. The sensation and effect it produces are extremely analogous to those which arise from excessive bleeding; the animal, under its influence, sinking from existence in the most placid swoon.

On the Parima, amongst the tribes the most celebrated for the use of the worari, I was told, that salt and sugar were considered as the best antidotes to this poison. The same was stated

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to M. de la Condamine, upon the Amazon; but afterwards, if I remember well, it was said to have been disproved by some experiments made in Germany.

I am, nevertheless, inclined to think, that some of the tribes do possess a secret antidote to the worari; for I was assured by the Portuguese, that the Indians of the Rio Negro are in the habit of shooting birds and monkeys with the worari, and afterwards resuscitating and transporting them to Para for sale.

This would be an interesting subject for a traveller to investigate. Could such an antidote be found, as to render the woorari manageable, I feel a persuasion that it would put us in possession of a most important medicinal agent in convulsive disorders, as in tetanus and hydrophobia, and in diseases perhaps of an acute inflammatory nature.

Does it kill by the privation of oxygen, the pabulum of the blood, and supporter of vitality?—If this were the *modus operandi*, by which it subverts the living power, its effects might possibly be restrained by inhaling the oxygenous gas, or by cautiously throwing oxygen into the veins.

Be this as it may, it is probable that the same principle belongs to very different plants. If so, an important discovery remains to be made—that of ascertaining the proximate principle, which, acting on the nervous and vascular systems, proves so subversive of animal life.

JOHN HANCOCK.

On a Method of Cultivating Plants in Walls, for Ornaments; with a Catalogue of those which succeed under this treatment.

TO THE EDITOR.

DEAR SIR,

FOR these ten years past, and more, I have been intending to communicate to you, for the sake of such of your readers as it might interest, a species of improvement (if I may use this grandiloquous term for want of a better) in one department of ornamental horticulture, which I had put in practice in different places long before this; yet I have delayed it till I need

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not delay it longer. I had thought that it might possibly be known to others as well as myself; and was unwilling to produce as a novelty what might not prove one; yet, having now communicated it to numerous persons, to all of whom it was unknown, and not having seen, in this country, a single attempt of the same nature, I suppose I may venture to presume that this notice will in reality present a novelty, and, slender as the fact is, a new source of ornament and amusement in a department which ought to despise nothing, since its sole ends are but amusement and ornament. The bare fact itself is known to every gardener, and even to botanists; but the application has been overlooked, or Art has neglected to profit by what Nature offers to its eyes every day.

I allude to the facility with which many plants, a great range, in fact, of even highly ornamental flowers, grow in or on walls; in many cases, even selecting them in preference, where the choice is left to themselves. And when I recall this familiar fact to horticultural readers, there are some who will, perhaps, immediately see the application here intended; but as none seem yet to have done that, I may be allowed to point it out.

In the ancient architectural gardens, masonry formed an essential ingredient; and, in a great measure also, it was necessary, that this masonry should be displayed, because it belonged to an architectural composition. We may regret, in passing, that the rage of innovation, too often hurrying from one fault to its opposite, has swept all this away; yet, though modern gardening has not only done this, but attempted to exclude all sight of such art, it cannot always and every where succeed. In many ancient establishments there are still subsisting remains at least of former ages, maintained through necessity or other causes, yet, in general, now producing only deformity, divested as they have been of all to which they once belonged, and, often, further associated with modern freedom, so as to produce effects scarcely less dissonant than would arise from an intermixture of modern and ancient fashions in dress. In other cases, walls are matter of necessity, for the mere purposes of defence or separation; or perhaps the wall of the fruit and kitchen-garden, unoccupied by fruit-trees, in-

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trudes itself disagreeably. Sometimes, a peculiar form in the ground about the house or garden renders walls, the support of terraces or otherwise, indispensable; and in similar places staircases of masonry are demanded for communication, while the abandonment of any architectural plan or structure in these, renders that a source of absolute deformity which was once turned towards the general good effect of the domain. Thus also may I point at the walls of hothouses, greenhouses, and so forth, often interfering with the beauty of the flower-garden, with which they are so frequently associated, and very especially so, where brick, and not stone, is the material.

But not to dwell further on these cases, it needs not be said that since the masonry about gardens has now abandoned all attempts at beauty, whether in the disposition, design, or execution, it is in almost every instance a deformity, and often to so great an extent as to injure materially the general effect of an ornamental garden, at least where taste has been called on to preside.

That indeed needs not be urged, since it is acknowledged in the attempts at concealment by means of trailing shrubs, the only remedy in use, yet not the only applicable one, and one also which, in some cases, cannot be applied; while, further, in others, the effect which it produces is not the best that could be obtained. There are many of the cases alluded to, such as in terraces and staircases, or in very low walls, inclosures, and narrow passages between walls, where trained shrubs are inconvenient or inapplicable. But not to enumerate all the objections to this system, there is also a question of taste here involved. In the first place, the number of shrubs capable of being thus trained is limited, and thus we are cramped in point of variety, and not less perhaps in that important circumstance in horticulture, the succession of flowers. And these plants are also all shrubs, which, though they include more than one rose, the jessamine, clematis, honeysuckle, and other flowers of great sweetness and beauty, form still but a limited list, and a list which, in any one place or spot, will always be small, from the great space which any one plant occupies.

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Nor is this the only question of taste. If a whole bare wall is always an ugly object, such masonry is far from being so under partial concealment, at least if of stone. Painters know well the value of such tints and such flat vacant surfaces, in enhancing the effects of form and colour; nor is there perhaps a local and limited, a formless, or shapeless object more engaging than the grey walls of an ancient abbey, when the interstices of the stones, the broken buttresses, the soffits, corbel tables, or whatever other "coin of vantage" give root to the wild plants which find their own lodgments there; to the ash, the ivy, the wallflower, antirrhinum, valerian, and even the tufts of grass, which mark the ruins of past days. The painter's study of such a ruin forms, in fact, the principle or basis on which the present proposal rests; it is to imitate nature in these dispositions, and thus to give interest and beauty to what was deformity. And if the utterly bare and naked wall of the ancient abbey or castle is deprived of more than half its beauty by that nakedness, no less than does the modern and necessary garden wall, thus bare, distress the eye of taste: so if no painter would hesitate in preferring the ruin thus partially ornamented with plants, to one where the whole masonry should be concealed by ivy, we may draw a similar conclusion as to that vulgar wall which is entirely concealed by trained shrubs, and that which is here proposed to ornament in a more sparing and varied manner; though, as to the cases in which the one or the other mode ought to be adopted, no rule can be given to guide what taste alone must direct for each case. Let those who may doubt, that greater beauty may often be produced in this manner, recollect, if they can, such ancient castle or abbey wall as they may chance to know, thus partially concealed, or else entirely covered with ivy; and after this, decide: and could one example of the bad effect of an universal green covering be of use, I might point out Restormel Castle, in Cornwall, as an instance, where the effect is thus totally ruined by that ivy which conceals every stone, and gives a magnificent specimen of ancient castellated architecture the semblance of a huge round bush.

I need not proceed further, as far as the questions of beauty and taste are concerned; since no rules can be given for the

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disposition of such plants, any more than for the comparative choice of trained shrubs, or scattered flowers and plants, or a mixture of both. In all cases it is a question of pure taste, or of effect; but it belongs to the department of the painter, rather than the gardener; and to him especially, the careful student in foregrounds, who best can judge of the power and effect of forms and colours near to the eye, and who alone can regulate those in Nature, as they would form the principle of distribution in his own imitations of her. Fortunate, indeed, would it be for the art of ornamental horticulture, if it were made a branch of the landscape painter's office, not of the gardener's; nor will the flower-garden and the shrubbery ever become what they may be rendered, until this is done, or till the ornamenting gardener shall add a knowledge of landscape, through the study and practice of art, to his other qualifications. That it is not so in this country, that such is not even suspected to be the true and only road to beauty of this nature, is a proof, among a thousand others, of the almost universal ignorance of art, the almost universal absence of real taste and knowledge, which, in spite of the as universal and daily pretensions to a knowledge of pictures, pervades the opulent, and the otherwise educated, in Great Britain. But I must proceed to another branch of this subject, to matters of detail.

That a very large catalogue of plants, including many very ornamental flowers, can and do grow out of the interstices of masonry, will shortly be seen in the subjoined catalogue; limited as that is, from being almost confined to our own native plants. The fact is familiar to botanists generally, though such a catalogue has never yet been made. And while they know also that many plants prefer the surfaces or crevices of rocks to the freer soil, it will be found that these are equally willing to grow out of walls. Yet as a matter of practice, a few words are requisite on the nature, on the structure or texture of the wall in which some plants will grow more freely than others. Such remarks must be here made general, as it would have immeasurably prolonged this paper to specify them as to each plant; a few occasional notes or remarks must serve the purpose of information as to some of the most remarkable.

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It might be inferred beforehand, that more plants would grow (to take extreme cases) from a wall of rude stone masonry laid in clay, as is not an uncommon practice where lime is scarce, and either pointed with lime or not, or from a wall built by *grouting*, than from a compact and well-laid piece of brick-work. Such is the fact; and it is also true, that many plants will grow on the surface or top of a wall, which will not root in the interstices among the plaster, from the accumulations of soil which so easily form in that part. But a very great number, and many of the most desirable, will grow in the interstices of even the most well-wrought wall, whether of brick or stone, and even where the cement is of the firmest quality, as I have put to the test of experience, times without number. And this is the fact, of which I have found our gardeners particularly incredulous; while I have never seen one such attempt made in this country, if I except my own.

This, however, is an essential point; and it is needful, therefore, to bestow a few further words on it. The process may commence with the very building of the wall, by laying the roots in the mortar as the work proceeds; roots of perennials, of course, as it is not worth while thus to labour for annuals. I need not point out all the plants which may thus be introduced, as gardeners will easily supply what I omit; but I may mention, that this plan succeeds with the whole genus of Dianthus for example, and that every pink or carnation that I have ever seen tried, has thus rooted itself. And, with this tribe, the effect is peculiarly pleasing and ornamental; as their proliferous quality enables them to produce large cushions from a single root, with which a wall can almost be covered, were it deemed expedient. The same practice succeeds with the Tussilago fragrans, with the Antirrhinums, Sedums, and others; but that which I omit may be easily conjectured, as those which I have not thus tried will also leave room for the endeavours of others. Of sowing seeds in the same manner I have less experience, having never been able to return to the only spot where I had the opportunity of fairly trying this method, yet I see no reason why it should not equally succeed.

For a wall already built, it is plain that another proceeding is required. In this case, I have opened the pointing with a

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chisel, sufficiently wide to admit the root easily, and penetrating so far as to reach the looser mortar of the interior. After this it is secured by means of fresh lime or clay, to guard against accidents until it has taken root; nor are the roots of such plants long in finding means of penetrating the firmest and best laid pieces of masonry; having thus succeeded in a brick wall, for example, where the cement was so stony that the bricks gave way to the chisel in preference to the lime. The power of the Tussilago fragrans, in this respect, is quite extraordinary, and even proceeds at times to more than hazard, to destruction; since I have seen one case where such a plant had found its way from a garden, through and through the wall, in fifty places and more, sending out an offspring at every joint, on both sides, and ultimately dislocating a stone wall of three feet in thickness, so as to be on the point of oversetting it.

It is in this manner that I have most frequently sown the seeds both of annuals and perennials, and with general success; and I need not dwell on that part of the subject, as I also need not prolong these remarks on the mode of conducting this sort of cultivation. I may only add, that in dry seasons or peculiar circumstances, it would be expedient to keep the plant moist until it is rooted; while it will not be difficult to find expedients for this purpose by means of water and strings, or otherwise.

Let me now make a few remarks on the following catalogue, as I am desirous to restrain this paper within as moderate bounds as possible. It is not solely a catalogue of our native plants, as far as they will grow in such situations, but, such as it is, I have introduced none that I have not actually seen thus growing, through the length of time in which I have paid attention to this subject. If it also contains every native plant that I myself have so observed, though I have no doubt that there are many more, and if I intended at first so to limit it, I could not do this; because there were some important plants capable of such treatment, and belonging to no native genus, and which therefore could not else have been pointed out. And this plan I have followed wherever the genus was native; that is, I have introduced, under the genus, only the

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native species, referring in a general manner to the foreign ones. My object in this was chiefly brevity; as I might otherwise have transcribed a large portion of our catalogue of cultivated plants. Thus, every Sedum which is hardy, and every Dianthus which we can cultivate, will grow in this manner; and to have quoted nominally the whole of such lists would have been tedious. A few genera, of which we have no examples, are named in a note, or alluded to as offering probabilities.

On the other hand, the catalogue is too long in one sense, because it includes many plants which no one would be at the trouble of cultivating. But I did not well know exactly where to stop in omitting; while, as the fact itself is a question of botanical physiology, I thought it inexpedient to sacrifice a catalogue once made; since that bare catalogue might, in another way, interest those who have attached themselves to our native Flora. I am not aware, finally, that I have omitted any thing necessary; and shall be pleased, if a suggestion so simple, and no less neglected, shall add any thing to the amusement or interest of those who occupy themselves with this elegant pursuit; shall lead to the display or production of one new beauty, or the concealment of one deformity. And if I could once have referred, for a full proof of the truth and the effects, to a flower-garden once constructed by myself at Dunkeld, I must regret that I can no longer command that; the whole of this particular part of that garden having been destroyed to give way to some alterations.

Catalogue of Plants which admit of being cultivated in and on Walls.

An asterisk * marks the few which are most deserving of cultivation.

Antirrhinum *, almost the whole genus
— cymbalaria
— elatine
— repens
— linaria
— orontium
— majus
— arvense
— minus

Antirrhinum spurium
With every other hardy one of this genus.
Aiva
— præcox
— flexuosa
Arabis
— thaliana
— stricta
— turreta

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Anthyllis
— vulneraria *
Arenaria
— trinervia
— verna
— rubra *
— serpyllifolia
— tenuifolia
Anethum
— fœniculus *
Apium
— petroselinum *
Aerostichum
— septentrionale *
Asplenium *, the genus
— geterach
— trichomanes
— viride
— ruta muraria
— adiantum nigrum
— lanceolatum
Adiantum
— capillus Veneris *

Under this letter I may name some of the genus Alyssum, and such of the Agave and Aloe as are sufficiently hardy, having witnessed their success. The American Aloe, as it is popularly called, may be thus managed in masonry so as to produce very pleasing effects.

Bromus
— mollis
— sterilis
— diandrus
Bellis
— perennis *
Borago
— officinalis *
Ballota
— nigra
Betula
— alba *

I have seen this tree growing to the height of twenty feet and more, out of a very thin and well-laid stone wall, without any communication with the earth.

Carduus
— lanceolatus
Cherleria
— sedoides *
Cotyledon
— umbilicus *
— luteum

With probably any foreign ones sufficiently hardy.

Cerastium
— vulgatum
— viscosum
— arvense
— semidecandrum
Cistus
— helianthemum *

And I believe every cistus, native, and otherwise hardy.

Cardamine
— hirsuta
Cheiranthus *, the genus
— fruticulosus
— sinuatus
— incanus

I have known only these species, but suspect that all the genus would succeed.

Cressis
— tectorum
Convolvulus
— sepium *
— arvensis *

And probably every hardy Convolvulus and Ipomea. Their creeping powers render them peculiarly applicable in certain cases.

Chrysanthemum
— leucanthemum *
Cochlearia
— officinalis
— greenlandica

In general not native.

Calendula
— vulgaris *, and
Crassula, wherever hardy, *

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Dianthus *, the whole genus
— barbatus
— prolific
— armeria
— caryophyllus
— deltoides
— cœsius
I have merely mentioned the species for which I can answer, but believe it true of all the genus, and it is particularly ornamental.
Draba
— verna
— hirta
— muralis
Euphorbia
— portlandica *
And, I suspect, many more.
Echium
— vulgare *
Erysimum
— barbaria
— alliaria
Erigeron
— acre *
Epilobium
— angustifolium *
Fumaria *, the genus
— capreolata
— lutea
— officinalis
— clariculata
Festuca
— myurus
— duriuscula
— ovina
— rubra
— bromoides
Fraxinus excelsior *
Like the birch, despising soil.
Fragaria
— vesca *
And especially capable of being rendered very ornamental in this manner.
Galium
— anglicum
Geranium *, the genus
— maritimum
— moschatum
— lucidum
— rotundifolium
— robertianum
— cicutarium
I suspect it also to be true of many foreign species.
Hordium
— murinum
Hieracium *
— murorum
— pilosella
— sylvaticum
Leontodon
— taraxacum
Lepidium
— petræum
— levigatum *
Lactuca
— virosa *
— scariola
In foreign and hardy plants, under this letter, I may point out Lavandula stœchas.
Medicago
— lupulina *
Matricaria
— parthenium *
Myosotis
— arvensis *
— versicolor *
Mercurialis
— annua
Origanum
— vulgare *
Ononis
— arvensis *
I find this noted, yet without accurately recollecting the particulars; and in foreign plants, the Oenothera mollissima, being very ornamental.
Plantago
— lanceolata
Prenanthis
— muralis *

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Parietaria
— officinalis *
Poa
— rigida
— compressa
Phalaris
— phleoides
Phleum
— paniculatum
— nodosum
Papaver *
— argemone
— rheas
— dubium
Pteris
— crispa *
Polypodium *
— vulgare
— fontanum
— cristatum
— rhæticum
— fragile
— dryopteris
Pinus
— sylvestris *
Growing to a considerable tree, like the birch, out of solid stone walls.
Rhodiola
— rosea *
Rosa
— spinosissima *
And suspected to be true of some others.
Rubus
— fruticosus *
— cæsius *
Probably more.
Reseda
— luteola *
— lutea *
Of plants not native, the Reseda odorata will also grow in this manner from seed, and produce triennial plants. I believe I may add the Rosmarinus officinalis among foreign species.
Sagina
— procumbens
— apetala
Statile
— armeria *
Solidago
— virgaurea *
— cambrica *
Sonchus
— oleraceus
Silene *
— nutans
— maritima
— armeria
— acaulis
Sempervivum
— tectorum *
And probably all that may prove hardy.
Sedum *
— album
— acre
— sexangulum
— anglicum
— dasyphyllum
— reflexum
— rupestre
And doubtless every hardy species in this genus. Many of them are very ornamental.
Sysimbrium
— tenuifolium
— murale
— sophia
— iris *
Senecio
— vulgaris
— squalidus
— viscosa
Salvia
— verbenaca *
Saxifreaga *
— tridactylitis
— hypnoides

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I have lost the note belonging to this genus, and will not conjecture now; but I believe many more, and especially the Alpine ones, will thus succeed; among others, the beautiful oppositifolia.

Teucrium
— scorodonia *
— chamædrys *
Tamarix
— gallica
Thalictrum
— majus *
— alpinum *
Thymus *
— acinos
— serpyllum
— nepeta

The serpyllum forms a singularly beautiful ornament.

I have already noticed the Tussilago fragrans, and may add that few flowers deserve cultivation better, from the singular fragrance of that which flowers when there is scarcely another appearing. This plant ought to be added to our Flora; growing in Guernsey in so many and such places, that it cannot have escaped from gardens, the more especially as it is there cultivated but in very few, and also but recently. Dr. Smith, indeed, while admitting those which he knew, says that the Flora of these islands has no more claim on a place in a British one than the Flora of Gibraltar; a somewhat singular comparison, it must be admitted, for a Briton, acquainted of course with the history of England, and of that country which was not the conquest, but the conquering side.

Veronica
— arvensis *
— verna *

Probably more.

Valeriana
— rubra *
— calcitrapa
— locusta
Verbascum *
— thapsus
— lychnitis
— pulverulentum
— nigrum
Vicia
— sylvatica *
Urtica
— diocea
— pilulifera

Among foreign genera, the hardy
Yuccas.

Such is the list of my experience, having been unwilling to go beyond that; but I may suggest one or two points for the consideration of those who may be inclined thus to amuse themselves.

I presume that almost every hardy Alpine which prefers rocks, such as some alyssums and saxifrages, would thus succeed. It is probable that similar success would attend such of the foreign plants as grow in dry sands; the mesembryanthemums, for example, should there be any hardy ones discovered, and that it would be true in general of all the succulent plants, which nature has contrived for these very

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ends. Lastly, an attention to the genera here named will indicate experiments on other species, or, as an example, if the Lychnis viscaria will thus thrive, so might the Chalcedonica.

Modern botanists may be shocked at some of the antiquated names here adopted; but the remarks were associated with names acquired before it became the essence of "the lovely science" to change its nomenclature once a year; and I saw no great necessity for consulting a table of synonimes which might be changed again before this paper was printed. A science of names cannot suffer much by such neglect, as long as there are catalogues; and it is probable that the majority of readers will still find themselves most at their ease in the fashion which is passed away among those who undertake to regulate the fashion of botany.

With your permission I will now add a postscript on a subject of an analogous nature, interesting for the same or similar reasons, yet to a somewhat different set of persons; namely, to the ever-longing and ever-disappointed horticulturists of cities and towns, whose gardens are a tea-pot or a flower-pot, emulous of the gardens of Adonis, a smoked balcony, a darkened and smoky area, containing a few square yards of grass or gravel, or the somewhat freer, yet still poisonous inclosure of a square. On this, however, I can do little more than suggest, or rather produce, a faulty and imperfect notice, as a stimulus to those who can do better, and to whom, perhaps, the having cause for blame will, as is common, prove the most engaging inducement. It is true, that professional gardeners are frequently consulted on this subject, by the anxious prisoner of towns longing for the sight of something that resembles the fair face of nature; and it is equally true, that such advice as they do give is limited, and often worthless. Yet there must be some gardener or horticulturist who knows incomparably more on this subject than I can pretend to do; and my end will be accomplished, if some such person will supersede a very bad catalogue by a very good one. If even he shall meet dispraise instead of thanks, he will have the satisfaction of having attempted to multiply the innocent amusements of his race.

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It is as unnecessary to point out the general antipathy which vegetables in general have to towns, as it is at present difficult to explain the cause. It would be somewhat extraordinary, indeed, if it were understood; when what is called the science of botany is solely occupied in making and changing names and arrangements, as if its objects were not only dead matter, but useless specimens of forms; and when the conventional, or perhaps necessary limitations of the far other valuable science of horticulture, together with the extent of this pursuit, and its no very remote origin as a science, seem to cut it off from the more refined anatomical and physiological inquiries necessary to illustrate this, and far more in the history of this great division of animated nature.

But, indeed, if the immediate or proximate cause is unknown, we are scarcely better informed as to the remote and acting one. It is not exclusively want of light, because as much light can be obtained in towns as in the country; and "want of air" is a term without meaning. If it is excess of carbonic acid, or indeed if it be any other derangement of the proportions in the constituents of the atmosphere, why cannot our refined chemistry detect this? It is said to arise from smoke, and, in our own towns, to depend exclusively or especially on coal smoke. Certainly this is not the exclusive cause; since similar effects take place in towns where wood is burnt, and where comparatively there is little smoke of any kind; nor is it easy to conceive how smoke acts, when we know what its nature is, and know that this very substance can be applied largely to plants in a solid state, or mixed with water, without injuring them in the same manner. It is probable, however, that the clue must be sought in that which has not simply been neglected, but denied; and that is, the sensations, the vital power, or nervous system of plants; denied by those who have, through all time, explained the actions of plants by mechanical principles, by the immense majority of botanists, or nearly by all; and in exactly the same deep philosophical spirit which, in the hands of a few others, assigned the actions of animals to similar causes.

But to pass what cannot at present be explained, it is an object of interest to trace the effects, be the cause, whether

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proximate or remote, what it may; as one, at least, of these concerns the purpose of this brief note. On this, however, I must content myself with a very few slight remarks, as I dare not prolong this postscript.

In London, as in Edinburgh and Glasgow, or other rapidly increasing towns, it is easy to follow the gradually widening circle of this noxious atmosphere, and in certain parts of those also its increase of noxious power. The receding of nurseries from the precincts of these towns is the evidence of the former; and, of the latter, he who will search, will find proofs enough, in the gradual extinction of plants which had gone on resisting through years. I know not, in London, a more distinct example of the last than in the garden of Mr. Bentham in Westminster, which, once bearing many fruits and flowers, even in abundance and perfection, is now gradually yielding to the increasing influence, and will probably soon be reduced to that limited number of plants which seem endowed with the power of resisting these effects. And it has not been uninteresting to trace the progress; the disappearance or non-production of the stone fruits there cultivated, having been among the first effects, and that (if, as I believe, I am correct) having been followed by their flowers; the currant and gooseberry afterwards suffering in the same order, and some or other of the flowering plants and shrubs, together with some trees, annually and successively becoming more enfeebled, or ceasing to live.

But, to omit a long detail, the downward progress of this garden will aid in illustrating the appended imperfect catalogue; though I must remark, that it does not afford a rule for all London, as the vicinity of the Park secures it probably from many consequences as to the tenderer town plants; just as Grosvenor and Lincoln's Inn squares are favourable to many species that would not exist in St. Paul's churchyard.

It would have been useful could we have discovered any general principle on which to determine beforehand what plants would succeed in these situations. And, perhaps, one might be found, should the investigation be pursued to a far greater increase of this meagre catalogue. At present it presents none, whether as relates to natural affinities, orders, or even genera,

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or as it concerns origin, climate, or hardiness to other injuries. One only remark, at all bearing on this question, has been made, and it is no less unexpected than remarkable. It is, that very many of the Alpine plants will thrive in the most confined allies, in the garrets of working mechanics for example, where almost every other plant dies; and without seeming at all affected by a change, so enormous in every sense, as to almost render the fact incredible. How far this sort of endurance extends through this geographical division of plants has not however been ascertained; but it offers one broad basis which will save much detail in the following catalogue.

Such as I have been able to make this catalogue, and chiefly from observations collected in Glasgow, the most smoky town in Great Britain, here it is; though I cannot pretend to say, out of all these, which are the most and which the least hardy in this sense. I have no doubt that many gardeners can materially enlarge it, and I must hope that some one will do so; supplying also that proportional scale, towards which I could but have added so few fragments, that I thought them better omitted altogether. These plants, of course, are ornamental ones, since ornament is the object; and I have used the most popular rather than the botanical names, as the end was that they should be generally understood. The latter are introduced only where there was no English popular name, or where that name itself was little known:—

Laburnum.
Lilac.
Hawthorn.
American Ivy.
Ivy.
Jessamine.
Lily of the valley.
Solomon's seal.
White lily.
Orange lily.
Yellow lily.
Turncap lily.
Periwinkle.
Both species.
Matricaria parthenium, or Feverfew.
Valeriana pyrenaica.
— rubra.
Bladder senna.
Alchemilla vulgaris.
— alpina.
Scilla nutans, or Wild hyacinth.
Statice armeria, or Thrift.
Scarlet bean.
Marigold.
Common bean.
Box.
Mignonette.
Sweet William.

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Mule pink, or
Dianthus.
In general, including all or most pinks and carnations.
Rocket.
Hollyhock.
Lavatera.
Elder.
Dogwood.
Poppies:
But it is remarkable that, if the seeds are sown in the border and in the gravel both, those in the border will often fail, while those in the gravel succeed.
Saxifraga hypnoides.
— oppositifolia.
And many more.
Alyssum.
More than one of this genus.
Sunflower.
Crocus.
Snowdrop.
Daffodil, and others of
Narcissus.
Aucuba japonica.
Mimulus ringens.
Wallflower.
Cheiranthus:
The whole of the stocks or gilliflowers, I believe.
Auricula.
Onopordum acanthium.
Mespilus pyracantha.
Yellow lupin.
Sweet peas.
Nasturtium;
Both species.
Convolvulus: tricolor, and more of this genus.
Gum Cistus:
Probably more of cistus.

Such Alpine plants as I have thought fit to omit may be added; and it is best perhaps to leave even the blank I might fill, that others may, in trying, add species of which I am ignorant. I *may add, that the vine continues to bear fruit* where the stone fruits have ceased, and even where the gooseberry and currants appear to be verging to an end.

Allow me yet to hope, before closing this paper, that some competent person will also favour the public, through your Journal, with a catalogue of such flowers as will succeed in thickets and woods, or within the shade and influence of our various trees and shrubs. Every one knows the disagreeable blank so often caused in these situations by the want of flowers or flowering shrubs; not seldom, by the entire absence of plants of any kind; and he who has busied himself in ornament of this nature, has often had to regret that he could not remedy this defect, nor obtain the requisite information. There are few gardeners probably who have not at some time been applied to for advice on this subject, as well as the former; and most assuredly the advice is not obtained, since the defect continues. It cannot be irremediable; and if the knowledge

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does not exist, it can at least be procured by the contribution of many, if not by the efforts of one. But as I am ashamed of the scantiness of my own list, deprived of the means of collecting one, I will not add a dozen or two of names, when I hope soon to see a far greater number.

Still there is another subject connected with ornamental gardening, which has been strangely neglected—strangely, in a climate like ours, and still the more remarkably, when it is recollected that the fashions of our country render winter a sort of conventional summer, as they also reverse the proposition; or that the time allotted to the rural residence is that in which all the brightest flowers of summer have disappeared; and, still more, that in which the great mass of vegetation is dormant or dead.

To the great bulk of the opulent, the flower garden and shrubbery, often far more, are lost to all but the gardener; it is ornament and expense, without comparative use. Why, then, are the autumnal and winter gardens neglected, while every thing is reserved for spring and summer? Who, among the higher classes, see the lilac and laburnum flower in their own grounds? How many see even the rose? Yet nearly two centuries are past since this recommendation, even to the details, such as the knowledge of that day could make them, was urged by Lord Bacon, urged, yet neglected, since there is scarcely a winter garden in Great Britain, and certainly not one such as might be constructed with a very small degree of attention.

The reason is not in the ignorance of gardeners, since they do possess knowledge enough of the plants which would serve this purpose; it must be sought partly in their neglect, but chiefly in the neglect, and more in the ignorance, of rural proprietors, who do not seem aware that such a thing is possible. Had they learned what and how to command, their servants would have learned to obey. And while such gardens may be constructed, it is surely superfluous to remark what pleasures might be derived from a spot which excluded the aspect of winter, even did it but cheat us with a cold semblance of summer. How this may be effected, and what are the evergreens and the successions of late flowers, it is not my purpose here

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to point out, having perhaps already protracted this paper beyond due bounds. But my wish is, that those whose proper business it is would publish such catalogues in those works which are in every one's hands, and direct the attention of proprietors and gardeners to those plants which, if known, are not pointed out to the ignorant; urging further the advantages of such an improvement, and adding such details respecting choice, disposition, succession, and so forth, as would here, and in hands like the present, be misplaced.

I am, &c.

J. Mac Culloch.

On the Construction of the Galvanic Battery.

I beg leave to present to the Editor of the Quarterly Journal an account of a galvanic battery invented by myself some years since, which has been adopted by several of my countrymen of the United States of America, and approved by many chemists in Paris, who have seen its operation.

The general description is this:—The copper plate is formed into a narrow cell in which the zinc is inserted and prevented from contact by bits of varnished wood. A number of pairs thus arranged are suspended from a common bar of wood by wires, the communication being made as usual between each zinc and the contiguous copper by a metallic slip. It is easy to see that by plunging the cells into a vessel containing the liquid until they are filled, and lifting them out, the instrument will be in action, which may be suspended by emptying them, and again renewed by filling them in the same manner. The facility of operation is greater than that of any other form I have ever seen, and a great power is saved by their complete insulation; in addition to which, the necessity of separate cells of glass, porcelain, &c. is superseded, which are expensive, troublesome, and fragile. The instrument may be constructed by any tin worker; and the zinc plates, when worn, can be renewed at a very trifling expense.

I shall pass on to the minute description, for which purpose I send drawings.

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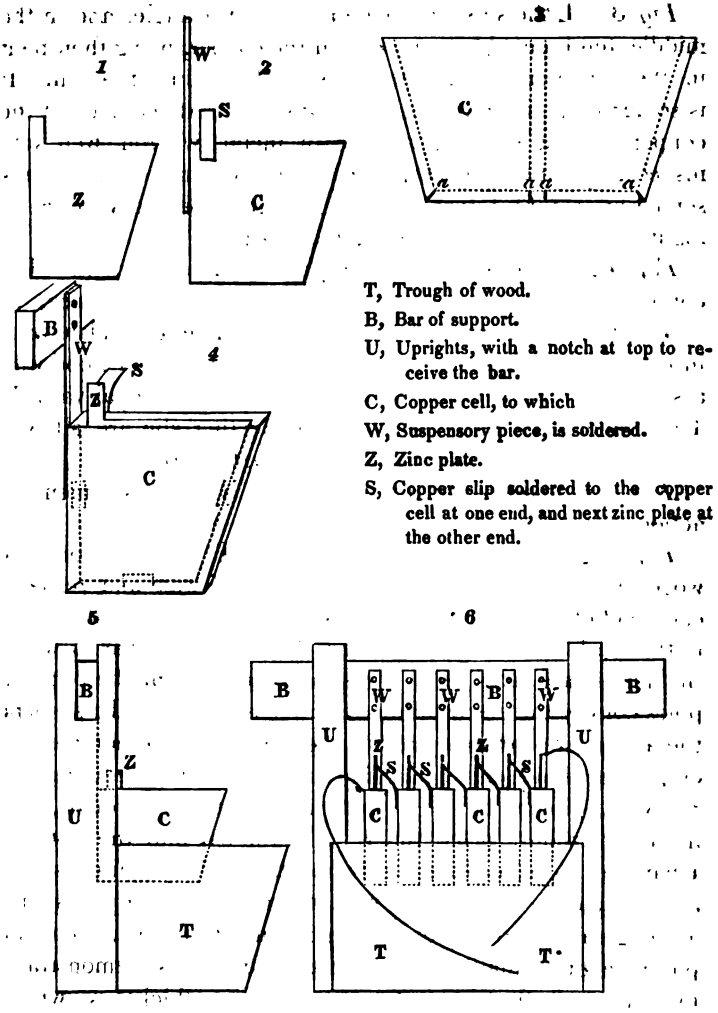

T, Trough of wood.
B, Bar of support.
U, Uprights, with a notch at top to receive the bar.
C, Copper cell, to which
W, Suspensory piece, is soldered.
Z, Zinc plate.
S, Copper slip soldered to the copper cell at one end, and next zinc plate at the other end.

Fig. 1 represents the form of the zinc plate, which is rhomboidal, with a projection at the upper part, to which is soldered one end of the slip connecting it with the next copper plate. It is made of rolled zinc.

Fig. 2 gives the form of the copper plate, which is formed into a cell open at top. In order to form it the copper should be cut in the shape given at

Fig. 3. Little slits are made at the lower angles and in the middle, represented by *a a a a*, and the copper, being then bent in the direction of the dotted lines, will produce the cell. It is soldered on the lower and sloping sides. At the back of the copper plate is soldered a piece of metal, w (iron, well varnished), by which it is suspended from the common bar represented by B. To one side also is soldered the slip s, connecting it with the zinc of the contiguous pair.

Fig. 4 gives a perspective view of a single element. The copper cell, c, is represented with its suspensory piece, w, attached to the bar, B, by two screws. The zinc plate z is inserted in it, and prevented from contact by bits of wood with a slit in one side, which have been boiled in copal varnish. The copper plate suspensory and connecting slip are all well varnished *exteriorly*, and the *soldered part interiorly*.

Fig. 5 represents an end view of the whole instrument in action.

Fig. 6 giving the front of the same. T being a trough of wood, well joined and lined with cement of wax and resin, at each end of which is an upright support of wood, with a notch in the top large enough to receive the end of the bar to which the plates are attached; wires proceeding from the opposite poles convey the electric fluid where it is wanted. To suspend the action, we have only to lift the bar out of the notches and empty the fluid either into the same or another trough. To renew it, plunge the cells until filled into the trough, and lifting them out, place the ends of the bar into the notches. I have constructed several instruments of different dimensions; and comparing their action with those upon other plans, find a very great superiority of force in favour of my own. The importance of insulation even for combustion is demonstrated by placing in the circuit a wire of a given thickness, which, while the plates remain immersed in the fluid, will show no sign of combustion, but when they have been lifted out, is instantly heated to a high degree. Should a plate prove defective, it may be replaced with but little trouble; and an immense power occupies but little space, the cells being only half an inch wide, and not more than a quarter from each other. The action also, being renewed or suspended at plea-

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sure, gives room for the professor to explain. If you should think this description worth inserting in your Journal, you will oblige

Yours respectfully,

ROBERT GREENHOW.

A short Account of Experimental Researches on the Diffusion of Gases through each other, and their Separation by mechanical means. By Thomas Graham, A.M., F.R.S.E., Lecturer on Chemistry, Glasgow.

FRUITFUL as the miscibility of the gases has been in interesting speculations, the experimental information we possess on the subject amounts to little more than the well established fact, that gases of a different nature, when brought into contact, do not arrange themselves according to their density, the heaviest undermost, and the lightest uppermost, but they spontaneously diffuse, mutually and equably, through each other, and so remain in an intimate state of mixture for any length of time. The beautiful illustrations of Mr. Dalton, by which this law was first developed, have rendered it familiar to every one. The subsequent experiments of Berthollet were made with uncommon care, and in most favourable circumstances, yet it is difficult to draw more from them than the same general fact; unless perhaps that hydrogen is much more penetrating and diffusive than any of the other gases*. It is sufficiently evident, however, from Berthollet's experiments, that, in cases of gaseous mixture which are exactly similar, corresponding results may be expected, or that the diffusion is not accidental, but subject to fixed laws.

In the prosecution of further inquiry into the laws of the diffusion or miscibility of gases, much use was made of a

* Berthollet's experimental paper is contained in the Mém. d'Arcueil, vol. i. p. 463; but the whole experiments are given in a tabular form in Dr. Thomson's System, vol. iii. p. 33.

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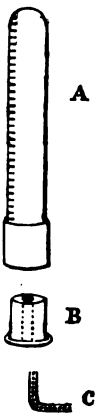
cylindrical glass receiver A, 9 inches in length, and 0.9-inch internal diameter, divided into 150 equal parts, and provided with a stopper B, fitted into the mouth of the receiver by accurate grinding. The stopper was perforated longitudinally, cavity cylindrical, 0.34-inch in diameter, and 1.8 in length. Into the cavity of the stopper there was again ground a short piece of stout tube, having a bore of 0.07 or nearly $\frac{1}{14.3}$ inch, and bent into a right angle in the middle; such as C. These were the dimensions of tube A; but after several experiments that tube was laid aside, and a second and a wider tube, of 0.12-inch bore and 2 inches in length, was ground into the aperture of the large stopper B, and bent in the middle, like tube C.



. . . .

I.—*On the Diffusion of the different Gases into atmospheric Air.*

The receiver, above described, was filled in succession with various gases in a state of purity, and supported in a horizontal position upon a frame, within a box, with the end of the bent tube pointing upwards, when the contained gas was heavier than air (*fig.* 1), and downwards, when the gas was lighter (*fig.* 2), to avoid any tendency of the gas to flow out of the receiver. After the gas had been allowed to diffuse into the air through the tube for a certain time, the receiver was transferred to the pneumatic trough, and the quantity of air which had entered, and gas that remained, ascertained. Two or three and sometimes more experiments were made on each gas, and the results found to be regular, or to vary within moderate limits.

Fig. 1.

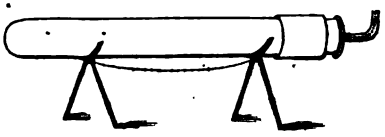


Fig. 2.

(1). After diffusion for ten hours, through tube I, there

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was found in the receiver, of which the capacity = 150 parts—of

Hydrogen gas (sp. gr. 0.0694·)	8.3 parts.
Carburetted hydrogen of marshes (sp. gr. 0.5555·)	56
Ammoniacal gas (sp. gr. 0.59027·)	61
Olefiant gas (sp. gr. 0.9722·)	77.5
Carbonic acid (sp. gr. 1.5277·)	79.5
Sulphurous acid (sp. gr. 2.2222·)	81
Chlorine (sp. gr. 2.5)	91

(2). After diffusion for four hours through tube I—in 152 parts there was found—of

Hydrogen gas	28.1
Carburetted hydrogen	86
Ammoniacal gas	89
Olefiant gas	99
Carbonic acid	104
Sulphurous acid	110
Chlorine	116

There have, therefore, left the receiver in the same time—of

Hydrogen gas	123.9 parts.
Carburetted hydrogen	66
Ammoniacal gas	63
Olefiant gas	53
Carbonic acid gas	48
Sulphurous acid	42
Chlorine	36

In deducing the comparative diffusiveness of the different gases from the table above, it is necessary to keep in mind the diminishing rate, according to which the latter portions of the gas leave the receiver. It was determined, with precision, in the case of olefiant gas, that that gas continues to leave a receiver, by diffusion, according to the same diminishing rate which holds in mechanical exhaustion by the air-pump. Hence the initial diffusions of the gases are even more varied than the numbers of the table. As much hydrogen gas left a receiver in two hours, as of carbonic acid in 10 hours. Hence the former gas is five times more diffusive than the latter. In all cases the gases were necessitated to diffuse in *opposition* to the solicitation of gravity. Yet carburetted hydrogen and ammoniacal gases left the receiver in greater proportions than

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olefiant gas did, although the diffusion of the former gases was more opposed by mechanical causes.

It is evident that the diffusiveness of the gases is inversely as some function of their density—apparently the square root of their density.

The results, however, are much influenced by the mechanical resistance arising from gravity, which is not constant in gases of different densities, the position of the receiver remaining the same. The effect of the position of the receiver may be conceived from an experiment on hydrogen gas. The receiver, filled with hydrogen gas, was placed in an upright instead of a horizontal position (see *figure*). Other circumstances being the same as in the experiment of table (1), of 150 parts hydrogen 22.1 were found remaining in the receiver after diffusion for ten hours, instead of 8.3 parts, as in that experiment.

Although the stoppers fitted precisely, the additional precaution of luting the joinings was attended to. The properties of the receiver, too, were found not to be peculiar to it.

II.—*On the Diffusion of mixed Gases into atmospheric Air.*

In the case of an intimate mixture of two gases, I was anxious to learn if each gas left the receiver, independently of the other, in the proportion of its individual diffusiveness—which would be a step gained in the solution of the important problem of the analysis of mixed gases by mechanical means.

For this purpose, the receiver was filled with 75 vols. hydrogen + 75 vols. olefiant gas, agitated and allowed to stand over water for 24 hours, that the mixture might be as perfect as possible. The receiver being then placed in the usual position, the mixed gases were allowed to diffuse into the air for ten hours. The receiver thereafter was found to contain

Hydrogen gas . . .	3.5
Olefiant gas	56.6
Air	89.9
	150.0

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There have left the receiver—of

Hydrogen gas	71.5 out of	75 parts.
Olefiant gas	18.4	75

The more diffusive gas has, therefore, separated from the other, and left the receiver in greatest proportion.

Now, when the receiver contains nothing but pure olefiant gas, 72.5 parts of that gas leave the receiver in the circumstances of the preceding experiment. Hence, when the receiver is half filled with olefiant gas, we would expect the half of 72.5 parts, or 36.25 parts to leave the receiver, and this happens when the complementary 75 parts are common air. But instead of 36.25 parts, only 18.4 olefiant gas leave the receiver in the last experiment. The disparity between the diffusion of each of the mixed gases, in that experiment, is actually greater than the disparity between the solitary diffusions of the same gases.

In the case of mixed gases, the law is—that the more diffusive gas leaves the receiver in a *greater* proportion than in the case of the solitary diffusion of the same gas, and the less diffusive gas in the mixture in a *less* proportion than in its solitary diffusion—a law of the diffusion of mixed gases, which was confirmed in upwards of forty experiments on diverse gaseous mixtures. Some of these experiments I shall subjoin.

(1.) The receiver was charged with

Carbonic acid	75	} = 150
Hydrogen	75	

which were allowed to mix intimately over-night. The mixture was afterwards allowed to diffuse into the air through the tube for ten hours. Position horizontal, mouth of tube downwards. Thereafter contained,

Carbonic acid	45
Hydrogen	4.65
Air	100.35
	150.00

In this experiment, a portion of the carbonic acid may have *flowed* out, for at the end of the experiment the density of the gaseous mixture was greater than that of the atmosphere, while the mouth of the tube opened downwards.

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(2.) Receiver charged with

Carbonic acid	102	= 152
Hydrogen	50	

With tube II. Position of receiver horizontal, mouth upwards. After diffusing into the atmosphere for four hours, contained,

Carbonic acid	76
Hydrogen	10.3
Air	65.7
	152

(3.) Receiver charged with

Carbonic acid	76	= 152
Carbur. hydrogen (of marshes)	76	

Tube II, mouth upwards. After four hours, contained,

Carbonic acid	57
Carbur. hydrogen	35.3
Air	59.7
	152

Have left the receiver,

Carbonic acid	19
Carbur. hydrogen	40.7

or, twice as much carburetted hydrogen as carbonic acid has left the receiver. Of these gases individually, there left the receiver in the same circumstances,

Of Carbonic acid	48
Carburetted hydrogen	66

(4.) Receiver charged with

Carbonic acid	52	= 152
Carburetted hydrogen	100	

Position, &c. as in preceding experiment. After four hours, contained,

Carbonic acid	39
Carbur. hydrogen	51.6
Air	61.4
	152

Have left the receiver,

Carbonic acid	13
Carbur. hydrogen	48.4

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(5.) Receiver charged with

Carbonic acid	31
Carbur. hydrogen	121 = 152

Position, &c. as above. After four hours,

Carbonic acid	23
Carbur. hydrogen	71
Air	58
	152

Have left the receiver,

Carbonic acid	8
Carbur. hydrogen	50

These three last experiments form a series. Suppose we had a mixture of two gases, of the same densities as carbonic acid and carburetted hydrogen, in equal volumes, but which could not be separated from each other by chemical means. Allow this gaseous mixture to diffuse for a certain time, as in Experiment 3, into a gaseous or vaporous atmosphere, which may afterwards be absorbed or condensed with facility. On condensing this atmosphere, there would remain a mixture, consisting of two parts of the light, and one of the heavy gas.

By a similar diffusion of the mixture thus obtained, we would procure a third mixture, consisting of four parts of the light, and one of the heavy gas, (Experiment 4.)

By a third diffusion, a mixture would be obtained of six or seven of the light, and one of the heavy gas, (Experiment 5.)

In this way a specimen of the light gas would at last be eliminated, by a species of rectification, in a state of tolerable purity.

On the other hand, if a specimen of the dense gas be desired, a converse series of operations must be pursued. What remains in the receiver after diffusion must be preserved, accumulated, and submitted again and again to diffusion.

(6.) Receiver was charged with

Olefiant gas	76
Carburetted hydrogen	76 = 152

After four hours, contained,

Olefiant gas	47.75
Carbur. hydrogen	41.40
Air	62.85
	152

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Have left the receiver,

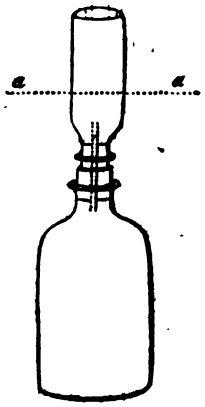
Olefiant gas . . .	28.35
Carbur. hydrogen . .	34.60

III.—*Diffusion of Gases into other Atmospheres than common Air.*

(1.) A phial, A, of 5.2 cubic inches, provided with a perforated cork, was filled with an intimate mixture of olefiant and hydrogen gases in equal proportions. The phial being held with its mouth undermost, a glass tube of 0.12 inch bore was thrust through the cork, and likewise quickly inserted into

Fig. 1.

Fig. 2.



the perforated cork of another bottle, B, of 37 cubic inches, containing carbonic acid gas. The whole was then sunk in water, till the surface of the water, *a a*, (*Fig.* 2.) rose above the joinings. After ten hours, the upper phial was removed, and its contents washed with lime-water. There remained a mixture, consisting of

Olefiant gas . .	12
Hydrogen . . .	3.1

There can be no doubt that the olefiant gas would have been obtained in a state of greater purity, had not the diffusion of

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the hydrogen gas been greatly impeded, 1st. From the direction in which it took place, downwards; and, 2dly. From the density of the medium into which it diffused.

Had the mixture of olefiant gas and hydrogen been allowed to diffuse *upwards*, and into an atmosphere of specific gravity intermediate between that of its constituent gases—into steam or ammoniacal gas, for instance, circumstances would have been most conducive to the unequal diffusion and separation of the mixed gases.

(2.) Hydrogen gas, in a tall receiver, is expanded by sulphuric ether, I find, four times more rapidly than common air. Mr. Leslie had already observed, that ice evaporates twice as rapidly in hydrogen gas as in common air; and he and Mr. Dalton found the cooling powers, or *mobility* of the different gases to be inversely as their density.

(3.) Gases permeate with increased facility in both directions through the pores of porcelain tubes at high temperatures (Priestley), because, I believe, their tendency to diffusion, which is inversely as their density, is vastly increased by their rarefaction, and not from any dilatation of the pores of the porcelain, which must be utterly trivial in the most intense heat.

(4.) A tall receiver was $\frac{3}{4}$ths filled with a mixture of 2 hydrogen + 1 oxygen, which had remained mixed for three weeks, but was found sensibly pure before the experiment. A little ether being thrown up into the receiver, the experimental mixture rapidly expanded. The first bubble projected from the receiver by the expansion was received, deprived of all ether-vapour by washing, and being exploded, left half its bulk of *pure hydrogen gas*.

(5.) The vapour of water appears, from the following experiment, to be more diffusive than the vapour of alcohol, as might be expected from the densities of these vapours. Of dilute alcohol (0·964), three ounces were exposed to spontaneous evaporation in a cylindrical jar two inches deep, and the same quantity in a jar six inches deep, but otherwise similar, the mouths of both jars being loosely covered with paper. When each of the vessels had lost half an ounce by evaporation, the remaining liquor was examined and found to contain sensibly more alcohol in the case of the deep than of

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the shallow jar. The difference, however, was altogether insufficient to enable us to account for the well known experiment of the concentration of alcohol in a bladder, by referring it to the superior diffusiveness of water-vapour. But it is conceivable, and the subject is at present under investigation, that imperceptible pores, or orifices of excessive minuteness, may be altogether impassable (by diffusion) by gases of low diffusive power, that is, by dense gases, and passable only by gases of a certain diffusive energy. Hydrogen gas certainly escapes from a bladder more rapidly than any other gas, and probably from diffusion, as the place of the hydrogen is found occupied by common air. But to these investigations, and to certain theoretic considerations, I hope again to recur in a future paper.

Observations on the Oxidation of Phosphorus. By Thomas Graham, A.M., F.R.S.E., Lecturer on Chemistry, Glasgow.

We are at present in possession of several curious facts respecting the insensible combustion of phosphorus at low temperatures.

1. In pure oxygen gas, under the atmospheric pressure, and at temperatures below 64°, the usual white smoke is not seen around phosphorus in day-light, and it is not luminous in the dark. No absorption of oxygen takes place.

2. A slight expansion of the oxygen gas, produced by diminishing the pressure upon it two or three inches below the usual pressure of the atmosphere, occasions phosphorus to be acted upon by pure oxygen, and to undergo slow combustion.

3. By diluting oxygen with certain gases, such as hydrogen, azote, protoxide of azote, carbonic oxide, carbonic acid, &c., the oxygen becomes capable of supporting the slow combustion of phosphorus even under the atmospheric pressure, as well as when rarefied by reduced pressure. Hence phosphorus is luminous in common air. The proportion of foreign gas necessarily varies according to the nature of the gas.

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4. Certain other gases do not qualify oxygen to act upon phosphorus at low temperatures, in whatever quantity they may be added to it. This is the case with olefiant gas, and with azote obtained by the action of a paste of sulphur and iron on common air.

The first and third of these facts have been known for a long time; the second was discovered by M. Bellani de Monza; and the fourth appears to have been first observed by M. Thénard (Traité de Chimie, t. i. p. 236, where the subject is treated at length).

In experimenting upon this subject, another curious fact was noticed. The presence of a minute quantity of certain gases and vapours entirely prevents the usual action of phosphorus upon the oxygen of common air. Thus the slow combustion of phosphorus does not take place at all, at the temp. of 66°, in mixtures of

	Volumes of air.
1 volume olefiant gas and . .	450
1 ditto vapour of sulphuric ether and	150
1 ditto vapour of naphtha and .	1820
1 ditto vapour of oil of turpentine and	4444

A stick of phosphorus was repeatedly left for upwards of 24 hours over water in air containing only one-four hundredth part of its bulk of pure olefiant gas, during the hot weather of July and August 1828, thermometer frequently above 70°, without diminishing the bulk of the air in contact. A slight expansion, amounting sometimes to $\frac{1}{100}$th part, occurred on several occasions. A stick of phosphorus, with a few drops of water, was corked up in a large retort, 213 cubic inches in capacity, and containing common air, with which $\frac{1}{20}$th of its bulk of pure olefiant gas had been mixed. During three months the phosphorus never became luminous, although its surface was gradually covered with a thin white crust. The water present was found to have become slightly acidulous.

The influence of a minute quantity of ether vapour, in extinguishing the combustion of phosphorus at low temperatures, may be exhibited in a striking manner. Introduce two or three moist sticks of phosphorus into a pint-stoppered phial, into which, when filled with the white fumes, pour a little ether,

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vapour from the ether bottle. In a few seconds the fumes entirely disappear, and the air around the phosphorus becomes perfectly transparent. If the bottle is now stopped, white fumes do not again appear in it, till the ether has passed entirely into acetic acid by combining with oxygen, which requires a few days.

Phosphorus is not luminous in the dark in air slightly impregnated with any other essential oil, as well as oil of turpentine. In an open two-ounce phial, phosphorus will appear brightly luminous in the dark; but the moment the phial is stopped with a cork, which has formerly confined an essential oil, and still sensibly retains its odour, the light begins to fade, and disappears entirely in a few seconds. The light from phosphorus in air at 63° F. is extinguished by the addition of 4 per cent. of chlorine gas, or 20 per cent. of sulphuretted hydrogen. The vapour from strong alcohol of about 80° in temperature extinguishes luminous phosphorus. But the vapours from camphor, sulphur, iodine, benzoic acid, carbonate of ammonia, iodide of carbon, do not produce that effect,—thermometer 67°. Held in the mouth of a bottle, containing strong muriatic acid, phosphorus appears to become more brilliant. But this is not the case with nitric or nitrous acids, which sensibly impair the light. The vapour from the liquor condensed in the vessels of the Portable Oil Gas Company, and coal gas, protect phosphorus from oxidation.

It is evident from these experiments, that phosphorus cannot be used to withdraw oxygen from gaseous mixtures, containing olefiant gas, or the different compounds of carbon and hydrogen allied to that gas. It may be employed as a test of their presence even in very minute quantity.

The influence of those gases in preventing the oxidation of phosphorus in air appears even at elevated temperatures. Phosphorus may be melted and kept for any length of time at 212°, without alteration, in air containing an equal volume of olefiant gas. In three parts air, with two parts sulphuric ether, phosphorus became faintly and transiently luminous in the dark at 215°,—weak lambent flashes, which disappeared entirely at 210°, and were repeatedly revived and extinguished by alternately elevating and lowering the temperature between

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these limits. A pretty strong combustion occurred at 240°. The following table exhibits the temperature at which phosphorus first becomes faintly luminous in the dark in air containing different gaseous substances :—

In 1 volume of air and	1	volume of olefiant gas, at	.	.	200° F.
3	„	2 „ vapour of ether, at	.		215°
111	„	1 , vapour of naphtha	.	.	170°
166	„	1 „ vapour of turpentine, at		.	166°.

The manner in which the influence of these gases is modified by barometric pressure is the most curious part of the subject. The proportion necessary to prevent combustion depends entirely upon the density of the gases. Thus, although less than one four-hundredth part of olefiant gas prevents the combustion of phosphorus, barometer 29 inches, phosphorus has been observed in a luminous state, under the pressure of half an inch mercury, in air containing so much as an equal volume of that gas.

In the following table the first column of fractions expresses the largest proportion of olefiant gas, in a mixture of air and that gas, which allows phosphorus to be luminous under the pressure placed against it. A greater proportion of olefiant gas extinguishes at that pressure.

PHOSPHORUS LUMINOUS.

Proportion of olefiant gas.	Olefiant gas	+	Air	Barometric pressure.
$\frac{1}{3}$	1	+	2	1·4 inches
$\frac{1}{5}$	1	+	4	2·3
$\frac{1}{10}$	1	+	9	3·2
$\frac{1}{20}$	1	+	19	5·0
$\frac{1}{30}$	1	+	29	10·3
$\frac{1}{40}$	1	+	39	12·1
$\frac{1}{50}$	1	+	49	16·5
$\frac{1}{100}$	1	+	99	25·5
$\frac{1}{200}$	1	+	199	26·4
$\frac{1}{450}$	1	+	449	29·0

Thermometer at 70°. When phosphorus is luminous above the mercurial column in a barometer tube at the greatest pres-

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sure possible for a particular mixture, a slight inclination of the tube from its vertical position, which has the effect of condensing the gas, extinguishes the light; while, on bringing back the tube to its vertical position, the phosphorus again becomes luminous.

The influence of other vapour on the oxidation of phosphorus at various pressures did not present any material differences from that of olefiant gas just detailed.

Naphtha and turpentine vapours appeared to lose their negative influence very rapidly as the pressure was reduced.

Carburetted hydrogen of marshes impedes to a certain degree, but does not altogether prevent, the oxidation of phosphorus. Its effect vanishes over a mercurial column of a few inches, a circumstance which will be attended to with advantage in removing, by means of phosphorus, the small portion of oxygen generally found in that gas.

The sulphuret of phosphorus and phosphuretted hydrogen gas are likewise protected from oxidation, to a certain extent, by olefiant gas, sulphuric ether, &c., although less powerfully than phosphorus, in proportion to their higher accendibility.

The oxidation of potassium appears likewise, from several comparative experiments, to be considerably retarded in dry air, containing a fourth or a fifth of its bulk of ether-vapour or olefiant gas, particularly of the latter. A piece of potassium, about the size of a pea, confined for a month in dry air, containing a fifth of its bulk of olefiant gas, was merely covered by a thin coating of grey oxide; while another piece of potassium, in similar circumstances, with the exception of the olefiant gas, was deeply penetrated with fissures of a kernel white.

The interference of those gases in preventing the oxidation of phosphorus, &c., is probably allied to the influence of the same and several other gases in preventing the accension of the explosive mixture of oxygen and hydrogen by the electric spark, first observed by Sir H. Davy (Essay on Flame), and since confirmed and investigated by Dr. Henry (Phil. Trans. 1824), and Dr. Turner (Edin. Phil. Journal, vol. xi.) Olefiant gas was found to act most powerfully, half a volume preventing the combustion of the explosive mixture, that is, defending the hydrogen from oxidation; and here, as in the

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case of phosphorus, the olefiant gas seemed to suspend the usual action between the supporter and combustible, without undergoing any change itself. If the nature of this *influence* of olefiant gas is the same in both cases, it forms a singular and interesting subject of inquiry, readily accessible in its most minute details in the case of phosphorus.

Notice of the singular Inflation of a Bladder. By Thomas Graham, A.M., F.R.S. E., Lecturer on Chemistry, Glasgow.

In the course of an investigation respecting the passage of mixed gases through capillary openings, the following singular observation was made.

A sound bladder with stopcock was filled about two-thirds with coal gas, and the stopcock shut; the bladder was passed up in this flaccid state, into a bell-jar receiver filled with carbonic acid gas, and standing over water. The bladder was thus introduced into an atmosphere of carbonic acid gas. In the course of twelve hours, instead of being in the flaccid state in which it was left, the bladder was found distended to the utmost, and on the very point of bursting, while most of the carbonic acid gas in the receiver had disappeared. The bladder actually burst in the neck, in withdrawing it from under the receiver. It was found to contain 35 parts of carbonic acid gas by volume in 100. The substance of the bladder was quite fresh to the smell, and appeared to have undergone no change. The carbonic acid gas, remaining without in the bell-jar, had acquired a very little coal gas.

The conclusion is unavoidable, that the close bladder was inflated by the insinuation of carbonic acid gas from without.

In a second experiment, a bladder containing rather less coal gas, and similarly placed in an atmosphere of carbonic acid gas, being fully inflated in fifteen hours, was found to have acquired 40 parts in 100 of this latter gas. A small portion of coal gas left the bladder as before.

A close bladder, half filled with common air, was fully inflated in like manner, in the course of 24 hours. The en-

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trance of carbonic acid gas into the bladder depends, therefore, upon no peculiar property of coal gas. The bladder, partially filled with coal gas, did not expand at all in the same bell-jar containing common air or water merely.

M. Dutrochet will probably view, in these experiments, the discovery of *endosmose* acting upon aëriform matter, as he observed it to act upon bodies in the liquid state. Unaware of the speculations of that philosopher at the time the experiments were made, I fabricated the following theory to account for them, to which I am still disposed to adhere, although it does not involve the new power.

The jar of carbonic acid gas standing over water, the bladder was moist, and we know it to be porous. Between the air in the bladder and the carbonic acid gas without, there existed capillary canals through the substance of the bladder, filled with water. The surface of water at the outer extremity of these canals being exposed to carbonic acid, a gas soluble in water would necessarily absorb it. But the gas in solution, when permeating through a canal, it arrived at the surface of the inner extremity, would rise, as necessarily, into the air in the bladder, and expand it. Nothing but the presence of carbonic acid gas within could prevent the disengagement of that gas. The force by which water is held in minute capillary tubes might retain that liquid in the pores of the bladder, and enable it to act in the transit of the gas, even after the pressure within the bladder had become considerable.

Account of an Apparatus for ascertaining the value of different Alkalis.

To the Editor of the QUARTERLY JOURNAL of SCIENCE, &c.

SIR,

I HEREWITH send you some account of an apparatus which I have employed for many years in ascertaining the value of the different alkalis of commerce; it is more simple and less liable to variation in its results than any with which I am acquainted, as proposed for the use of persons not familiar with the niceties of chemical analysis. You will observe that I

have taken the principle of its formation from the paper of M. Decroizelle, in the Annales de Chimie.

I am, Sir,

Respectfully yours,

W. G. COLCHESTER.

London, Aug. 28, 1829.

The apparatus consists of a glass jar about one inch in diameter, containing about five cubic inches, and graduated into inches and tenths; a dropping tube about seven or eight inches long, divided into thirty equal parts; a porcelain mortar and pestle; a weight of 100 grains, and a bottle of sulphuric acid, so diluted that the quantity contained in twenty-two divisions of the dropping tube will just saturate fifty grains of crystallized sub-carbonate of soda. To determine the point of saturation litmus paper may be used, or, what is much more convenient, infusion of cabbage.

METHOD OF USE.

The sample to be examined having been pounded sufficiently to pass through a coarse sieve, rub up some of it in the porcelain mortar until it be reduced to a very fine powder; from this weigh 100 grains and return it into the mortar; add thereto boiling water, a small quantity at a time, and continue to rub it as long as any grittiness appears under the pestle; suffer it to stand a short time, and pour off the liquid into a pint or half-pint vessel with a lip; add more boiling water to what remains, and again use the pestle, repeating this to ensure the perfect solution of all the soluble part of the sample, until about half a pint of boiling water has been employed; transfer the whole into the same vessel, stir it well together, and allow it to stand for the insoluble part to subside; when this is effected, measure off the clear liquor by pouring it into the graduated jar and set it by for use; measure also the remainder, first shaking it up, and having noted the total quantity, this remainder may be thrown away. Take of the clear solution just one half of the whole amount of the two quantities, and add thereto about a table-spoonful of the infusion of cabbage; then, having filled the dropping tube to the upper

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division with the test acid, drop so much into the sample, constantly stirring the mixture, as will just change its green colour to crimson; the quantity of acid used, as indicated by the divisions on the tube, will show the per centage of alkali in the sample, if it be barilla, kelp, or manufactured soda; but, if the sample be pot or pearl ashes, augment the proportion of test acid used, by adding to the number of divisions indicated by the dropping tube, one half such number, and the total will be the per centage of alkali in such sample.

Should it be desired to ascertain the quantity of carbonic acid contained in the sample, we need only note the point at which the solution becomes blue in the foregoing process, and deduct the divisions then indicated by the test tube from the subsequent total amount; every ten of the remainder will then indicate seven per cent. of carbonic acid, whether of barilla or of pot-ash.

The apparatus is made and sold by Mr. Bate, Philosophical-instrument Maker, 21, Poultry.

Memoir on the Mean Results of Observations; read before the Academy of Sciences, April 20, 1829.

By M. Poisson.

This Memoir is the continuation of that which I inserted in the *Additions à la Connaissance des Temps* for the year 1827. My object is to add some new developements to that part of it which treats on the probability of arithmetical means between the results of a great number of observations. When there is no reason for believing some more exact than the rest, the mean of them should be taken for the unknown value sought; and one is naturally led to think that this mean result approaches the nearer to the truth as the number of the observations is more considerable. But La Grange is the first person who subjected this question to mathematical analysis*, and who investigated the probability that the arithmetical mean between any number of observations does not differ from the

* Vol. v. of the old Memoirs of the Academy of Turin.

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tude value by a quantity greater than some assigned limit. To solve it, he supposes known the law of probability of error in the observations, or the values which they may give for the thing which it is sought to determine; an hypothesis which prevents the formulæ deduced from it being applicable and of any use in practice. It is to Laplace we are indebted for having rendered the probability of the mean result independent of this law in the cases where the observations are numerous, so that from the sole numerical data of the observations the probability may be calculated of a determinate limit of error to be apprehended in taking this result for the value of the unknown quantity. I hope the details on this subject, into which I have entered in my Memoir, will be well calculated to dissipate the doubts which might still remain as to the degree of approximation of this probability*.

To form a precise and general idea of the limit to which the mean result of observations approaches indefinitely in proportion as the number of them increases, we must suppose the construction of a curve, the ordinates of which are proportional to the probabilities of the values of the unknown quantity, which last is expressed by the corresponding abscissa. If the law of probability change from one observation to another, this curve will change also; and another will be constructed, the ordinates of which will be means, for each abscissa, between those of all the particular curves. This being so, the limit in question, in every case, is the abscissa which corresponds to the centre of gravity of the area of the mean curve. This limit to which the mean result of the observations converges, is not necessarily one of the values of the unknown quantity which have the most probability, and are given most frequently by isolated observations. It may even happen that the probability is altogether none, and that it cannot be given by any single observation; which, in fact, will be the case if the ordinates of all the curves of probability are null for the same abscissa, and symmetrical on each side of it. In the general case, where the curve of probability varies from one observation to another, it may also happen that the areas of all the curves

* Supplement to Théorie Analytique des Probabilités, p. 7.

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may not have their centres of gravity on the same ordinate; the abscissa which corresponds to the centre of gravity of the mean area will then vary with the number of observations; and if this number be divided into several parts, which still consist of considerable numbers, the mean results of these partial series will not be the same, although the error to be apprehended from each of them is very small, and all possess a very great probability.

The calculation of mean life is one of the most ingenious applications which has been made of these principles. A great number—a million, for example—of children are considered as born at the same epoch, and the future duration of the life of each infant is assimilated to an eventual gain, of which the probability is unknown. The sums of all the possible durations of life, from zero to the greatest age which men can attain, multiplied by their respective probabilities, and relative to this infant, will then form his chance or his hope of life: consequently, mean life will be the sum of these quantities for all the infants divided by the number of them; now it is easy to see that this quotient is nothing else than the abscissa of the centre of gravity of the mean area, which was mentioned above. Thus, by taking the mean time that an equal number of individuals, born in the same country as the children under consideration, have lived, and at a period as near as possible to that of their birth, we shall obtain an approximate value of mean life; and from these observed durations of human life may be calculated the probability that this value does not differ from the truth, such as it has been defined, by more than a given time. The probability of living to an assigned age is, doubtless, not the same for a million of infants born at the same period. But it may be admitted that the mean of its unknown values varies but slowly by the extinction of maladies and the improvement of society; experience alone can teach us if this mean law of probability, and consequently the mean duration of life, remains stationary, or sensibly varies in long intervals of time.

It is also by the same principles that the mean advantage, and the probability of it, which may be expected from a very

great number of speculations, is calculated, from the known losses and gains of another very considerable number of similar operations, that is to say, of which the mean law of probability is supposed the same.

In other questions depending upon the same theory, where the subject is the greatness of a phenomenon or the measure of any thing which is to be determined by a series of observations, it is supposed implicitly that, among all the values of which this thing is susceptible by its nature, there exists one from which it is equally probable that there will be an equal difference in excess, or defect in each observation; it is supposed, moreover, that this value of the unknown quantity is the same for all the observations, and it is this value, thus defined, that it is sought to discover. That amounts to saying, that the curves of probability relative to all the observations are symmetrical on each side from one of their points, and that this point corresponds to the same abscissa for all these different curves. In this hypothesis the centres of gravity of their areas and that of the area of the mean curve will be situated on a common ordinate, the abscissa of which will represent the true value of the unknown quantity. By multiplying the observations, the quantity by which we shall indefinitely approximate will be constant and independent of their number; and although their laws of probability may be different, their mean result will give a value nearer and nearer to the unknown quantity; and at the same time, from the whole of the observations collectively, the probability of its degree of approximation may be calculated. But however small may be the error to be apprehended in taking the mean result for the value of the unknown, and however probable may be the limit of this error, it must not be lost sight of that the value of anything drawn from observations is always subordinate to the hypothesis already stated. If any unknown cause render the errors of the instruments or the variable circumstances which influence the phenomena, preponderant one way or the other, or if the thing to be determined varies progressively during the continuance of the observations, this hypothesis will not hold good, and the observations should be rejected as improper for

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this determination. It will therefore be important to ascertain by the observations themselves, if they are incompatible with the supposition that has been made, that is to say, with the symmetry of all the curves of probability on each side of a point corresponding to the same abscissa: now, in fact, conditions do exist which the observations should satisfy if this symmetry really have place.

Let us suppose, for the sake of illustration, that the mean result of a great number of observations be successively taken away from the particular results of each of them, which will make known their differences each way from the mean, which will be in general very small quantities, positive or negative, the sum of which will be nothing. If we take the sum of any powers of these errors, neglecting the signs, and the sum of the double of those powers, it is evident that the ratio of the first sum to the second will be inversely as the greatness of the errors, and consequently a very considerable quantity. Also, if the square root of the second sum be taken, the ratio of the first to this root will also be a large number of the order of the square root of the number of observations; that is, if there be, for example, a million of observations, the ratio in question will be comparable to one or to several thousands; but it will not be the same where the first sum is composed of uneven powers, and that their changes of sign are considered. This circumstance will diminish this sum; and the calculus shews that in the hypothesis of an equal probability of equal errors, plus or minus, the ratio of the sum of their uneven powers to the square root of the sum of the double powers, must be an inconsiperable fraction: we find, for example, that there is one against one to wager that the observations are incompatible with this supposition, when the ratio shall equal a fraction not differing much from $\frac{2}{3}$; thus, in comparing the sum of the cubes of the errors to the square root of the sum of the sixth powers, or the sum of the fifth powers to the square root of the sum of the tenth powers, &c. when we find for one of these ratios a fraction which is not much below $\frac{2}{3}$, that will suffice to shew the hypothesis in question to be improbable, and, consequently, that the observations ought to be rejected, as was stated above.

In a great number of cases, and particularly in questions of

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astronomy, the quantity which it is proposed to determine by observations, is a given function of several elements which are already approximately known, and in which it is only necessary to make very small corrections, the products of which and all the powers higher than the first are neglected. The given function then becomes a linear function of these unknown corrections. It is made equal successively to all the values resulting from experiment, which affords as many equations of condition as there are observations. The employment of these linear equations for determining the corrections of the elements, by adopting for the purpose a great number of observations, has contributed much to the improvement of the astronomical tables. It appears that Euler and Mayer are the first who employed them; one in his Memoir on the Perturbations of Saturn and Jupiter, which received the prize in 1750 from our academy, and the other in his Memoir on the Libration of the Moon. But their number being always superior to that of the unknown quantities, the solving of them occasioned some embarrassment, and this serious inconvenience ensued, that the calculators could deduce, from the same system of equations, different results according to the method of calculation they employed. When there was only one unknown quantity to be determined, it was agreed to render its coefficient positive in all the equations that were subsequently added, to form the final equation, from which the value of the unknown quantity was to be deduced. When these unknown quantities were two or more in number, the combination of the equations of condition that was made to reduce them to an equal number of final equations was absolutely arbitrary. This embarrassment, and the inconvenience which resulted from it, remained to the period when M. Legendre proposed a direct and uniform method of forming the final equations, which was generally adopted under the name of *Method of least squares of the errors*, which was assigned to it by its author. It consists, as is well known, in deducting from the result of each observation the linear function of which it furnishes an approximate value: the difference is the error of observation; the sum of the squares of all these differences is taken: its differentials taken successively, with respect to the corrections of all the elements,

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are there made equal to zero; which gives as many equations as there are unknown quantities to be determined. This method, if it possessed only the advantage of uniformity, and of freeing the steps of the calculation from all that is indeterminate, would be an important service rendered by our brother academician to the sciences of observation; but it is also that which leaves the minimum of error to be apprehended in the value of each element, as Laplace has proved by the calculus of probabilities. Let us add, in conclusion, that after having calculated the corrections of the elements by the method of least squares, and having substituted their values in the linear expressions of the errors of the observations, if we take the sum of the uneven powers of all these errors, and divide it by the square root of the sum of their double powers, the magnitude of the quotient will furnish a criterion, according to which observations should be rejected, or their results adopted, if they have, in other respects, a sufficient probability.

On a Method of rendering Platina malleable. The Bakerian Lecture. *By the late* William Hyde Wollaston, M.D. F.R.S., &c.

[From the Philosophical Transactions for 1829. Part I.]

As, from long experience, I probably am better acquainted with the treatment of Platina, so as to render it perfectly malleable, than any other member of this Society, I will endeavour to describe, as briefly as is consistent with perspicuity, the processes which I put in practice for this purpose, during a series of years, without seeing any occasion to wish for further improvement.

The usual means of giving chemical purity to this metal, by solution in aqua regia and precipitation with sal ammoniac, are known to every chemist; but I doubt whether sufficient care is usually taken to avoid dissolving the iridium contained in the ore, by due dilution of the solvent. In an account which I gave in the Philosophical Transactions for 1804, of a new metal, Rhodium, contained in crude platina, I have mentioned this precaution, but omitted to state to what degree the acids

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should be diluted. I now therefore recommend, that to every measure of the strongest muriatic acid employed, there be added an equal measure of water; and moreover, that the nitric acid used be what is called "single aquafortis;" as well for the sake of obtaining a purer result, as of economy in the purchase of nitric acid.

With regard to the proportions in which the acids are to be used, I may say, in round numbers, that muriatic acid, equivalent to 150 marble, together with nitric acid equivalent to 40 marble, will take 100 of crude platina; but in order to avoid waste of acid, and also to render the solution purer, there should be in the menstruum a redundance of 20 per cent. at least of the ore. The acids should be allowed to digest three or four days, with a heat which ought gradually to be raised. The solution, being then poured off, should be suffered to stand until a quantity of fine pulverulent ore of iridium, suspended in the liquid, has completely subsided; and should then be mixed with 41 parts of sal ammoniac, dissolved in about five times their weight of water. The first precipitate, which will thus be obtained, will weigh about 165 parts, and will yield about 66 parts of pure platina.

As the mother-liquor will still contain about 11 parts of platina, these, with some of the other metals yet held in solution, are to be recovered, by precipitation from the liquor with clean bars of iron, and the precipitate is to be redissolved in a proportionate quantity of aqua regia, similar in its composition to that above directed to be used: but in this case, before adding sal-ammoniac, about 1 part by measure of strong muriatic acid should be mixed with 32 parts by measure of the nitro-muriatic solution, to prevent any precipitation of palladium or lead along with the ammonio-muriate of platina.

The yellow precipitate must be well washed, in order to free it from the various impurities which are known to be contained in the complicated ore in question; and must ultimately be well pressed, in order to remove the last remnant of the washings. It is next to be heated, with the utmost caution, in a black-lead pot, with so low a heat as just to expel the whole of the sal-ammoniac, and to occasion the particles of platina to cohere as little as possible; for on this depends the ultimate ductility of the product.

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The gray product of platina, when turned out of the crucible, if prepared with due caution, will be found lightly coherent, and must then be rubbed between the hands of the operator, in order to procure, by the gentlest means, as much as can possibly be so obtained of metallic powder, so fine as to pass through a fine lawn sieve. The coarser parts are then to be ground in a wooden bowl with a wooden pestle, but on no account with any harder material, capable of burnishing the particles of platina*; since every degree of burnishing will prevent the particles from cohering in the further stages of the process. Since the whole will require to be well washed in clean water, the operator, in the later stages of grinding, will find his work much facilitated by the addition of water, in order to remove the finer portions, as soon as they are sufficiently reduced to be suspended in it.

Those who would view this subject scientifically should here consider, that as platina cannot be fused by the utmost heat of our furnaces, and consequently cannot be freed, like other metals, from its impurities, during igneous fusion, by fluxes, nor be rendered homogeneous by liquefaction, the mechanical diffusion through water should here be made to answer, as far as may be, the purposes of melting; in allowing earthy matters to come to the surface by their superior lightness, and in making the solvent powers of water effect, as far as possible, the purifying powers of borax and other fluxes in removing soluble oxides.

By repeated washing, shaking, and decanting, the finer parts of the gray powder of platina may be obtained as pure† as

* The following experiment will prove the necessity of attending to this precaution:—if a wire of platina be divided with a sharp tool in a slanting direction, and, being then heated to redness, be struck upon an anvil with a hammer, so as to force into contact the two newly-divided surfaces, they will become firmly welded together; but if the surfaces have previously been burnished with any hard substance, the welding will be effected, if at all, with very great difficulty.

When the powder of platina has been over-heated in decomposing the ammonio-muriate, or has been burnished in the grinding, I have in vain endeavoured to give it a welding surface, by steeping it in a solution of sal-ammoniac in nitric acid.

† Sulphuric acid, digested upon the gray powder of platina, thus purified, extracted less than $\frac{1}{1000}$th part of iron.

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other metals are rendered by the various processes of ordinary metallurgy; and, if now poured over, and allowed to subside in a clean basin, a uniform mud or pulp will be obtained, ready for the further process of casting.

The mould which I have used for casting is a brass barrel, 6⅜ inches long, turned rather taper within, with a view to facilitate the extraction of the ingot to be formed, being 1.12 inches in diameter at top, and 1.23 inches at a quarter of an inch from the bottom, and plugged at its larger extremity with a stopper of steel, that enters the barrel to the depth of a quarter of an inch. The inside of the mould being now well greased with a little lard, and the stopper being fitted tight into the barrel by surrounding it with blotting-paper, (for the paper facilitates the extraction of the stopper, and allows the escape of water during compression,) the barrel is to be set upright in a jug of water, and is itself to be filled with that fluid. It is next to be filled quite full with the mud of platina; which, subsiding to the bottom of the water, is sure to fill the barrel without cavities, and with uniformity,—a uniformity to be rendered perfect by subsequent pressure. In order, however, to guard effectually against cavities, the barrel may be weighed after filling it, and the actual weight of its contents being thus ascertained, may be compared with that weight of platina and water which it is known by estimate that the barrel ought to contain*. A circular piece of soft paper first, and then of woollen cloth, being laid upon the surface, allow the water to pass, during partial compression by the force of the hand with a wooden plug. A circular plate of copper is then placed upon the top, and thus sufficient consistency is given to the

* From the mean weight of the ingots obtained in previous operations, it is known that the barrel described in the text ought to contain 16 ounces troy of dry platina powder. The weight of the contents of the barrel $= 16 \text{ ounces} \times \frac{\text{sp. grav. of platina} - 1}{\text{sp. grav. of platina}} +$ the weight of a cubic inch of water $\times$ capacity of the barrel in cubic inches $= 16 \text{ ounces} \times \frac{20.25}{21.25} + .526 \text{ ounces} \times 7.05 = 18.9575$ ounces troy. Should the contents of the barrel weigh materially less than this estimated weight, there must be a want of uniformity in the disposition of the powder within the barrel.

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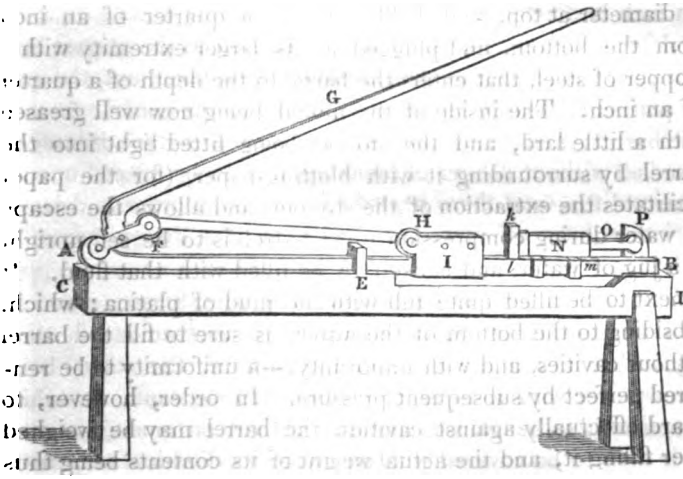
contents to allow of the barrel being laid horizontally in a forcible press.

The press which I have generally used for this purpose consists of a flat iron bar AB, set edgeways, and screwed down by a hook E, near its middle, where it would otherwise be liable to bend, to a strong wooden bench CD. The bar is connected by a pivot at its extremity A, with the lever



AFG. An iron rod FH, which turns at its two extremities upon the pivots F and H, proceeds from the lever at F, and, as the lever descends, propels forward the carriage I, which slides along the bar. A stopper or block being placed in the vacant space I *k*, the carriage communicates motion to the cradle *k l m*, which is also made to slide along the bar, and carries the barrel N, which lies upon the cradle, straight against the piston O, which rests by its end against P, a projection in the further extremity of the bar.

The weight, which in this machine, when the angle of the lever's elevation is small, will keep the power, applied vertically at the extremity of the lever, *in equilibrio* = that power $\times \frac{AG \times FH}{AF\,[AF+FH]} \times$ cotan of the angle of the lever's elevation; which expression, in the case of the press actually used, becomes, Power $\times$ 5. cotan of the angle of the lever's

elevation. This expression, at an elevation of 5°, becomes nearly 60 × power, and at an elevation of 1°, becomes nearly 300 × power; and when the lever becomes horizontal, the multiplier of the power becomes *quasi* infinite. This explanation will be sufficient to show the mechanical advantage with which, by means of this press, the weight of the operator, acting on the end of the lever, will be made to bear against the area of the section of the barrel, a circle little more than an inch in diameter.

After compression, which is to be carried to the utmost limit possible, the stopper at the extremity being taken out, the cake of platina will easily be removed, owing to the conical form of the barrel; and being now so hard and firm that it may be handled without danger of breaking, it is to be placed upon a charcoal fire, and there heated to redness, in order to drive off moisture, burn off grease, and give to it a firmer degree of cohesion.

The cake is next to be heated in a wind-furnace; and for this purpose is to be raised upon an earthen stand about 2½ inches above the grate of the furnace, the stand being strown over with a layer of clean quartzose sand, on which the cake is to be placed, standing upright on one of its ends. It is then to be covered with an inverted cylindrical pot, of the most refractory crucible ware, resting at its open end upon the layer of sand; and care is to be taken that the sides of the pot do not touch the cake.

To prevent the blistering of the platina by heat, which is the usual defect of this metal in its manufactured state, it is essential to expose the cake to the most intense heat that a wind-furnace can be made to receive, more intense than the platina can well be required to bear under any subsequent treatment; so that all impurities may be totally driven off, which any lower temperature might otherwise render volatile. The furnace is to be fed with Staffordshire coke, and the action of the fire is to be continued for about twenty minutes from the time of lighting it, a breathing heat being maintained during the last four or five minutes.

The cake is now to be removed from the furnace, and being placed upright upon an anvil, is to be struck, while hot, on the

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top, with a heavy hammer, so as at one heating effectually to close the metal. If in this process of forging, the cylinder should become bent, it should on no account be hammered on the side, by which treatment it would be cracked irremediably; but must be straightened by blows upon the extremities, dexterously directed, so as to reduce to a straight line the parts which project.

The work of the operator is now so far complete, that the ingot of platina may be reduced, by the processes of heating and forging, like that of any other metal, to any form that may be required. After forging, the ingot is to be cleaned from the ferruginous scales which its surface is apt to contract in the fire, by smearing over its surface with a moistened mixture of equal parts by measure of crystallized borax and common salt of tartar, which, when in fusion, is a ready solvent of such impurities*, and then exposing it, upon a platina tray, under an inverted pot, to the heat of a wind-furnace. The ingot, on being taken out of the furnace, is immediately to be plunged into dilute sulphuric acid, which in the course of a few hours will entirely dissolve the flux adhering to the surface. The ingot may then be flattened into leaf, drawn into wire, or submitted to any of the processes of which the most ductile metals are capable.

The perfection of the methods above described, for giving to platina complete malleability, will best be estimated by comparing the metal thus obtained, in respect of its specific gravity, with platina, which has undergone complete fusion; and by comparing it, in respect of its tenacity, with other metals possessing that quality in the greatest perfection.

* The chemist will find this flux very serviceable for removing from his crucible or other vessels of platina those ferruginous scales with which, after long use, and particularly after being strongly heated in a coal or coke fire, they become incrusted. In the analysis of earthy minerals, I have been in the habit of using a similar flux, composed of two parts by weight of crystallized carbonate of soda, and one of crystallized borax, well ground together. It has the advantage of not acting, like caustic alkali, upon the platina crucible, and is a powerful solvent of jargon and many other minerals, which yield with difficulty to other fluxes. If the mineral to be operated on requires oxidation, in order to decompose it, a little nitre or nitrate of soda may be added.

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The specific gravity of platina, drawn into fine wire, from a button which had been completely fused by the late Dr. E. D. Clarke, with an oxy-hydrogen blowpipe, I found to be 21.16. The aggregate specific gravity of the cake of metallic mud, when first introduced into the barrel, exclusively of moisture, is about 4.3; when taken from the press, is about 10. That of the cake fully contracted, on being taken out of the wind-furnace before forging, is from 17 to 17.7. The mean specific gravity of the platina, after forging, is about 21.35, although that of some rods, after being drawn, is 21.4: but that of fine platina wire, determined by comparing the weight of a given length of it with the weight of an equal length of gold wire drawn through the same hole, I find to be 21.5, which is the maximum specific gravity that we can well expect to be given to platina.

The mean tenacity, determined by the weights required to break them, of two fine platina wires, the one of $\frac{1}{3000}$, the other of $\frac{1}{3850}$ of an inch in diameter, reduced to the standard of a wire $\frac{1}{10}$th of an inch in diameter, I found to be 409 pounds; and the mean tenacity of eleven wires, beginning with $\frac{1}{4500}$ and ending with $\frac{1}{23.000}$ of an inch, reduced to the former standard, I found to be 589 pounds; the maximum of these eleven cases being 645 pounds, and the minimum 480 pounds. The coarsest and the finest wire which I tried present exceptions, since a wire of $\frac{1}{1500}$ of an inch gave 290 pounds, and a wire of $\frac{1}{30.000}$ of an inch, 190 pounds. If we take 590 pounds, as determined by the eleven consecutive trials, to be the measure of the tenacity of the platina prepared by the processes above described, and consider that the tenacity of gold wire, reduced to the same standard, is about 500, and that of iron-wire 600, we shall have full reason to be satisfied with the processes detailed in the present paper, by which platina has been rendered malleable.

To this paper I beg to subjoin an account of some processes relating to two of the metals which are found in the ore of platina.

To obtain malleable palladium, the residuum obtained from burning the prussiate of that metal is to be combined with

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sulphur, and each cake of the sulphuret, after being fused, is to be finally purified by cupellation, in an open crucible, with borax and a little nitre. The sulphuret is then to be roasted at a low red heat, on a flat brick, and pressed, when reduced to a pasty consistence, into a square or oblong and perfectly flat cake. It is again to be roasted very patiently, at a low red heat, until it becomes spongy on the surface. During this process, sulphur flies off in the state of sulphurous acid, especially at those moments when the heat is allowed occasionally to subside. The ingot is then to be cooled; and when quite cold, is to be tapped with a light hammer, in order to condense and beat down the spongy excrescences on its surface. The alternate roastings and tappings (or gentle hammerings) require the utmost patience and perseverance, before the cake can be brought to bear hard blows; but it may, by these means, at length be made so flat and square, as to bear being passed through the flatting-mill, and so laminated to any required degree of thinness.

Thus prepared, it is always brittle, while hot, possibly from its still containing a small remnant of sulphur. I have also fused some palladium *per se*, without using sulphur; but I have always found it, when treated in this way, so hard and difficult to manage, that I greatly prefer the former process.

To obtain the oxide of osmium in a pure, solid, and crystallized state, I grind together, and introduce, when ground, into a cold crucible, 3 parts by weight of the pulverulent ore of iridium, and 1 part of nitre. The crucible is to be heated to a good red in an open fire, until the ingredients are reduced to a pasty state, when osmic fumes will be found to arise from it. The soluble parts of the mixture are then to be dissolved in the smallest quantity of water necessary for the purpose, and the liquor thus obtained is to be mixed, in a retort, with so much sulphuric acid, diluted with its weight of water, as is equivalent to the potash contained in the nitre employed; but no inconvenience will result from using an excess of sulphuric acid. By distilling rapidly into a clean receiver, for so long a time as the osmic fumes continue to come over, the oxide will be collected in the form of a white crust on the

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sides of the receiver; and there melting, it will run down in drops beneath the watery solution, forming a fluid flattened globule at the bottom. When the receiver has become quite cold, the oxide will become solid and crystallize. One such operation has yielded thirty grains of the crystallized oxide, besides a strong aqueous solution of it.

On Achromatic Telescopes. By Signor G. Santini, Director of the Observatory at Padua.

Since the publication of my theory of optical instruments, Mr. Rogers has read to the Astronomical Society of London, a Memoir, which appears to be of the highest interest for optical science, but with which I am unacquainted, except by an extract inserted in vol. v. of a periodical work published at Vienna, by MM. Ettingshausen and Baumgartner, entitled "Zeitschrift für Physik und Mathematik," p. 120. Mr. Rogers, considering that the principal obstacle to constructing achromatic object-glasses for large refractors, is the difficulty of obtaining large pieces of pure homogeneous flint-glass, free from striæ, and thus fit, by being combined with another lens of crown glass, to produce at once an achromatic lens, has entertained the happy idea of interposing between the lens of crown glass and its focus, when the pencil of luminous rays is much contracted, a correcting lens composed of two smaller lenses of crown and of flint glass, brought into contact, which, retarding the convergence of the red rays, and removing further off that of the violet rays, produces, in the rays of mean refrangibility, the effect of a plane glass; and it is clear that correct images will be produced, if the surfaces of the correcting lens and the distance from the greater object lens be so arranged, that all the heterogeneous rays parallel to the axis be united in the mean focus of this last. The eminent author lays down a simple rule for determining the focal distances of the correcting lenses; and then observes, that if these be constructed, *nearly* according to the dimensions laid down in the rule, the remaining chromatic errrors may be destroyed by means of a micrometrical motion, by which the two smaller

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lenses conjointly can be brought nearer or removed from the first; and the errors arising from the figure will be destroyed by a small motion tending to separate in a small degree the two correcting lenses, without its being necessary, as in the ordinary theory, to retouch the surface of the last lens.

The simplicity of this construction made me curious to calculate numerically the dimensions assigned by theory, so as to verify the simple method which is given for destroying the remaining chromatic and spherical aberrations. Having, in this last particular, obtained results which do not exactly agree with the statements of the illustrious author, I have brought them together here, with the results of my calculation, from which it will more clearly appear, under what circumstances, and with what precautions, recourse must be had to the projected correcting lens.

Imagine a system of three lenses placed in the same axis, and constructed of crown and flint glass, the indices of mean refraction of which are respectively m, m'; the first and the second being of crown glass and convex; the third of flint and concave. Let their focal lengths be p, q, r, and $p = 1$; also, let the distances of the points of union of the rays be indicated respectively by a, α; b, β; c, γ; the distance of the first from the second lens $= d$; let the second and the third be imagined in contact. As the correcting lens should produce in the rays of mean refrangibility the effect of a plane glass, $q + r$ will be $= 0$, that is, $q = -r$. Now, considering that the rays parallel to the axis, a case which takes place in the object-glasses designed for astronomical observations, will be $a = \infty$, $\alpha = p = 1$, $b = -(1 - d)$; and assuming, for the sake of brevity, $\zeta = \frac{d\,m'}{d\,m}, \frac{m-1}{m'-1}$, the equation, which should exist to destroy the longitudinal aberration of refrangibility, will be

$$1 + \frac{b^2}{q}(1-\zeta) = 0; \text{ from whence } q = b^2(\zeta - 1);$$

$r = -b^2(\zeta - 1)$; whence d and b being assumed arbitrarily, the focal distances of the two correcting lenses will be obtained, from which the rule laid down by Mr. Rogers evidently follows. b, q, r, being obtained, the other distances β, c, γ, are

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readily ascertained, since $\beta = \frac{b\,q}{b-q}$, and the last lenses being in contact, c will be $= -\beta$, and since $r = -q$, γ will $= -b$. The figure of the lenses, such that the longitudinal spherical aberration may be destroyed, remains to be determined. For this purpose, let λ, λ', λ'', denote arbitrary numbers, on which their figure depends; and, for brevity (as in my Teoria degli Strumenti Ottici, vol. i. No. 104), let $\mu = \frac{m\,(4m-1)}{8\,(m-1)^2\,(m+2)}$, $\nu = \frac{4\,(m-1)^2}{4m-1}$, $\rho = \frac{4+m-2m^2}{2\,(m+2)\,(m-1)}$, $\sigma = \frac{m\,(2m+1)}{2\,(m+2)\,(m-1)}$, $\tau = \frac{m\sqrt{(4m-1)}}{2\,(m+2)\,(m-1)}$; $\mu', \nu', \rho', \sigma', \tau'$, denoting similar functions when the index m relative to the crown glass changes into the index m' relative to the flint. The proper reductions being then made, it will be found that the equation given in No. 108 of the above cited work, in order that the longitudinal spherical aberration in a system of three lenses may be destroyed, is brought, in the present case, to the following:—

$$\mu\lambda + \frac{b^2}{q^2}(\mu\lambda' - \mu'\lambda'') + \frac{b^3}{q\beta}(\mu\nu - \mu'\nu') = 0 \quad \ldots\ldots\ldots (d),$$

in which are the three arbitrary quantities, λ, λ', λ''; two of them, however, being determined at will, the third follows; when, from the well known principles of optics, the rays for each of the surfaces of the lenses will be obtained.

NUMERICAL EXAMPLE.

To construct an achromatic object glass, from the foregoing principles, with crown and flint glass of Frauenhofer's manufacture, of which the following are the indices:—

For the crown mean rays $m = 1.530000$
„ red rays $m - dm = 1.521000$ } $dm = 0.009$

For the flint mean rays $m' = 1.634494$
„ red rays $m' - dm' = 1.616707$ } $dm' = 0.017787$

From which we get $\zeta = 1.650853$. Having assumed $d = \frac{2}{3}$; b will be $= -\frac{1}{3}$, from whence $q = 0.072317$, $r = -0.072317$, $\beta = -c = +0.0594247$, $\gamma = -b = 0.3333333$. With respect to the figure of the lenses, two of the three quantities, λ, λ', λ'', remaining arbitrary, we shall determine λ, λ', so that the first

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two lenses shall be isosceles, which will be done by means of the following equations (No. 106, vol. i.):—

$$\sqrt{(\lambda-1)}=\frac{\alpha-\rho}{2\tau}\quad\sqrt{(\lambda'-1)}=\frac{\alpha'-\rho}{2\tau}\cdot\frac{b-\beta}{b+\beta}$$

From these, in the present case, we shall obtain $\lambda=1.6000676$, $\lambda'=2.233782$; from whence equation (a) will give $\lambda''=2.917899$. According to these numbers, the radii of the lenses will come out as follows:—

1st lens, $R=R^{I}=1.06$.

2d lens, $R^{II}=R^{III}=0.0766555$; double convex.

3d lens, of flint glass, double concave: $R^{IV}=-0.0800292$, $R^{V}=-0.1075404$.

The aperture should be determined so that that of the correcting lenses does not exceed the half of their focal length. The aperture of the first lens will thus be about $=0.108$: in round numbers $=0.1$, which exceeds remarkably what is adopted in practice for the larger object-glasses.

Let us now see, first, how in this system the chromatic aberration is destroyed for the rays nearest to the axis; wherefore let us assign for the first lens 0.002; for the second, 0.002; for the third, 0.001; and let k, k^{I}, k^{II}, k^{III}, k^{IV}, k^{V} represent the distances at which the rays nearest the axis unite after refraction at the first, second . . . sixth surface. From the principles of dioptrics we shall find,

For the mean rays.		For the red rays.	
$k=3.000000$	$k^{III}=0.0589577$	$k=3.094550$	$k^{III}=0.0603491$
$k^{I}=0.939673$	$k^{IV}=0.1809460$	$k^{I}=1.016946$	$k^{IV}=0.1823450$
$k^{II}=0.1542802$	$k^{V}=3.14150$	$k^{II}=0.1575919$	$k^{V}=0.314422$

So the remaining chromatic aberration will be $=0.000272$.

Before proceeding to calculate the spherical aberration, it is as well to destroy this remaining aberration by a small variation δd given to the distance d. Calling δk^{V} the correspondent variation of k^{V}, there will be found $\delta k^{V}=-\frac{k^{V\,2}}{(k'-d)^{2}}\delta d$; wherefore we shall have,

For the mean rays, $k^{V}=0.314150-0.889959\,\delta d$

For the red rays, $k^{V}=0.314422-0.805748\,\delta d$

Making these two values of k^{V} equal to each other, there will

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be $\delta d = -0.003230$; wherefore $d = 0.663437$. Recalculating the values of k by this corrected value of d, we obtain the following results:—

Mean rays.		Red rays.	
$k = 3.060000$	$k^{iii} = 0.0590609$	$k = 3.094550$	$k^{iii} = 0.0603006$
$k^{i} = 0.999673$	$k^{iv} = 0.1815420$	$k^{i} = 1.016946$	$k^{iv} = 0.1828973$
$k^{ii} = 0.1547307$	$k^{v} = 0.317137$	$k^{ii} = 0.1580190$	$k^{v} = 0.317921$*

Remaining aberration = 3.000016

from which it results that the remaining chromatic aberration may be neglected.

For the purpose of determining the spherical aberration which takes place in this system of lenses, with the superior distance d corrected, we shall denote the angles of incidence of a luminous ray which goes toward the extremity of the first lens in successively refracting surfaces, by $i, i^{i}, i^{ii} \cdots i^{v}$; the refracted angles respectively by $l, l^{i}, l^{ii} \cdots l^{v}$; the inclinations of the directions of the refracted ray to the axis, by $o, o^{i}, o^{ii} \cdots o^{v}$; the distances of the point of meeting from the respective refracting surfaces by $k, k^{i}, k^{ii} \cdots k^{v}$. Then retaining the above quantities for the size of the lenses, and in round numbers regarding $i = 2° 43'$, we shall find,

For the mean rays.

i	=	2	43	0.0	o^{i}	= 2	53 7.96
l	=	1	46	30.8	k^{i}	= 0.995676	
o	=	0	56	29.2	i^{ii}	= 9	39 45.36
k	=	3.0585405			l^{ii}	= 6	17 53.85
i^{i}	=	3	38	30.40	o^{ii}	= 6	14 59.77
l^{i}	=	5	36	9.16	k^{ii}	= 0.1539007	
i^{iii}	=	18	56	27.6	o^{iv}	= 5	14 45.7
l^{iii}	=	29	46	38.1	k^{iv}	= 0.1813396	
o^{iii}	=	17	5	10.3	i^{v}	= 3	32 54.9
k^{iii}	=	0.0529159			l^{v}	= 5	48 23.0
i^{iv}	=	29	12	51.8	o^{v}	= 2	59 17.6
l^{iv}	=	17	22	27.2	k^{v}	= 0.316238	

For the red rays.

i	=	2	43	0.0	o^{i}	= 2	50 11.3
l	=	1	47	8.6	k^{i}	= 1.012955	

* This should be .317121.—Ed.

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For the red rays.

	°	′	″		°	′	″
o =	0	55	51.4	i^{III} =	10	8	43.80
k =	3.0930420			δ^{II} =	6	39	1.45
i^{I} =	3	38	52.63	o^{II} =	6	19	53.65
δ^{I} =	5	33	12.53	k^{II} =	0.1571542		
i^{III} =	19	28	52.3	o^{IV} =	2	23	30.0
δ^{III} =	50	28	50.9*	k^{IV} =	0.1825478		
o^{III} =	17	19	51.2	i^{V} =	3	42	26.3
k^{III} =	0.0538743			δ^{V} =	6	0	1.9
i^{IV} =	29	53	44.8	o^{V} =	3	5	54.4
δ^{IV} =	17	57	23.6	k^{V} =	0.315527		

For the mean rays nearest to the axis k^{V} is = 0.317137, which, compared with the above value of k^{V} for the remote mean rays, will give the remaining spherical aberration, = 0.000899; that is, about ten times greater than what writers on optics lay down as tolerable to the eye.

Mr. Rogers extols, as one of the advantages of his new construction, the power of removing the spherical aberration by a slight separation of the second from the third lens, which is not to be defined, but to be performed by means of a micrometrical adjustment, until it is found by observation to be destroyed, or at least rendered unappreciable to the eye. Although it does not appear very commendable to allow a micrometrical motion to this system of lenses, from which may arise errors of centering more dangerous than those it is sought to avoid, it does not appear to me possible, *at least in this example*, to remove, by this method, the spherical aberration. In fact, introducing a small distance $\delta d'$ between the second lens and the third, the chromatic errors are reproduced which had been previously destroyed. We must, therefore, on this account, make the distance d vary contemporaneously by a small arbitrary quantity, δd: calculating numerically the coefficients of δd, $\delta d'$ in the expression of k^{V}, as well for the proximate as the remote rays, I obtain the following results:—

For the nearest mean rays, $k^{V} = 0.317137 - 0.88962\,\delta d - 28.8332\,\delta d'$
For the nearest red rays, $k^{V} = 0.317121 - 0.80472\,\delta d - 27.5364\,\delta d'$
For the remote mean rays, $k^{V} = 0.316238 - 1.21610\,\delta d - 34.8746\,\delta d'$

* So in the original, but this evidently should be 30° 28′ 50.9″.—Ed.

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Making an equation between the values of k^v, we obtain $\delta d = +0.014013$, $\delta d' = -0.0009021$; and the correcting lenses being previously supposed in contact, the negative result is inadmissible. I do not wish to deduce generally from this, that with no other ratio of dispersion the chromatic and spherical aberrations may not even this way be destroyed; only it appears that they vary considerably with the distance $\delta d'$; so that, if even in other cases the thing be possible, the micrometrical apparatus should be constructed with the greatest care.

Not having succeeded, by varying the distances, in destroying the spherical aberration, I have had recourse to small arbitrary variations, $d\,R^{iv}$, $d\,R^v$, given to the rays of the last lens, which I have regarded as positive, although relative to a concave lens, having with greater facility obtained in this hypothesis the general equations relative to the path of a ray of light. In this way I have found the following equations of condition.

For the nearest mean rays, $k^v = 0.317137 - 10.07444\,dR^{iv} - 5.51794\,dR^v$
For the nearest red rays, $k^v = 0.317121 - 9.79024\,dR^{iv} - 5.37508\,dR^v$
For the remote mean rays, $k^v = 0.316238 - 12.15546\,dR^{iv} - 4.97661\,dR^v$

The resolution of which gives $d\,R^{iv} = -0.0002655$; $d\,R^v = +0.0006401$; $k^v = 0.316280$. From whence the corrected values of R^{iv}, R^v will be $R^{iv} = 0.0797637$; $R^v = 0.1081805$; the distance remaining $d = 0.663437$.

If now the distance k^v of the point in which the rays unite behind the last lens be directly calculated in this system, there will be found—

For the mean rays nearest the axis,	$k^v = 0.316145$
For the red rays nearest the axis,	$k^v = 0.316136$
For the mean remote rays,	$k^v = 0.316120$
For the red remote rays,	$k^v = 0.315466$

Remaining aberrations.

Chromatic in the mean rays,	$= 0.000009$
Spherical in the mean rays,	$= 0.000025$
Spherical in the red rays,	$= 0.000669$

The first two remaining chromatic and spherical aberrations are extremely minute, and without the small errors from the tables to seven places of figures, or without the influence of the

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second powers of the corrections found, would have been evanescent; the last, that is the spherical aberration in the red rays, is about the same order as that which remains in the double object glasses, constructed according to the theories of Herschel and Frauenhofer; and therefore less than what remains in the triple object-glasses. It cannot be taken away altogether, but may still be diminished, if it would be worth while to give up the simplicity of construction which lenses of equal curvature present, such as we have supposed the first and the second to be. But it is evident, at the same time, that the aberrations vary considerably with the rays of the last lens; and hence the construction of a correcting lens of this sort requires great care, and should not be recommended, except for the greater telescopes designed for astronomical observations, for which they may be employed with great advantage and saving of expense. Another great advantage from the construction of Mr. Rogers is, the field being free from a coloured edge. In fact, the condition that should have place in a system of three lenses separated by the distances d, d', and disposed round the same axis, that the coloured margin should be removed, is represented by the equation (Vol. ii. No. 246)

$$d' = \frac{q+r}{2\frac{q}{d}},$$

which in this case is identical, since $d'=0$; $q+r=0$.

Such a combination, moreover, deserves all the attention of practical opticians.

GIOVANNI SANTINI.

Travels in Turkey, Egypt, Nubia, and Palestine, &c., in 1824, 5, 6, and 7. By R. R. Madden, Esq., M.R.C.S. 2 vols. London, 1829.

AT a moment when political events, of a magnitude scarcely to be estimated, are rivetting the attention of the world upon Turkey and Turkish interests, comes forward this, among other works, calculated to satisfy the literary craving which such a state of things is sure to engender. But, independent of the interest which Mr. Madden's work possesses from the peculiar circumstances of the times, it is well worthy of attention from the curious and obviously faithful picture which it

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presents of the condition of mankind in that extensive region which owns the sway of the Grand Signior. The author engages in the task with no ordinary claims upon our confidence. His was no rapid survey of a country, taken with pen in hand, where impressions are noted down as they first arise, unchecked and uncorrected—where much is trusted to second-hand information—where matters are treated of, foreign to the author's usual habits of observation and thinking. We have, in the instance before us, a gentleman, bred to the profession of physic, and though not yet possessed of its highest honours, well skilled in the knowledge which entitles him to them, sojourning for four years in the country which he professes to describe, with an ardent love of information, a good understanding, an unprejudiced judgment, and abundant opportunities of exercising all his faculties of observation and research. Such a man can hardly have failed of producing a book creditable at least to himself, and useful to others; but Mr. Madden's volumes have the additional merit of a high degree of literary polish. His style is easy and flowing. His graver reflections on literature and science are pleasingly mixed up with playful anecdotes, descriptions of scenery, and the little incidents of his own journey. It is impossible to read the work without feeling an interest for its author. His accuracy is unquestionable. His good humour never deserts him. This is all for which he takes credit. Apologizing, with much modesty, for his literary qualifications; acknowledging that he did not carry in his head Herodotus and Hamilton, Pococke and Pausanias, affecting not to be a learned traveller, he lays claim only to some share of patience and philosophy. An unruffled temper and a cheerful demeanour he has found to be the best passports to Turkish confidence. A mild manner and quiet deportment will, he says, carry a traveller through difficulties, which peevishness and pride (the too frequent qualities of an English traveller) would have rendered intolerable. The ferocity and fanaticism of the most obstinate Turk may be subdued, as it would appear, by good humour.

The volumes before us contain a vast fund of information respecting the present political and social condition of the inhabitants of the various provinces of Turkey,—European, Asiatic, and African. These, however, we do not propose to analyse at present. An ample field of observation remains in the medical and philosophical parts of Mr. Madden's travels. As a medical man, Mr. Madden had ample opportunities of ascertaining the state of literature, and more especially of me-

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dicine, in the countries through which he travelled, and he appears to have availed himself of them most fully. It shall be our object to lay before the reader the substance of the author's observations on these important topics, following, however, our own arrangement of the matter. It may be proper to premise, that Mr. Madden's Travels are written in the form of letters to his principal friends, whom he addresses on the subjects adapted to their respective capacities. His female acquaintances are entertained with the description of a Turkish toilette, and an occasional poetical effusion. The Reverend Mr. M'Pherson receives copious details concerning the Route of the Israelites, and the passage of the Red Sea. A letter to Thomas Coltman, Esq., barrister, affords the opportunity of describing the state of Turkish law, and its formidable adjuncts, the sack, the bowstring, and the bastinado; while his medical friends, Dr. James Johnson, Dr. Gregory, and Mr. Joshua Brookes are favoured with accounts of vapour-baths, amulets, and *madjouns*, the dysentery, the ague, and many such plagues and pestilences.

The volumes teem with accounts of the low condition to which medical science is reduced throughout Turkey; and such probably it was throughout all Europe not many centuries back. It has been well remarked, that the state of medicine may be considered as the criterion or barometer of the state of science in any country. Wherever science and refinement have extended their influence, there will medicine be most cherished, as being so eminently conducive to the interests and happiness of mankind. The following lively sketch of the mode of conducting *business* at Constantinople will illustrate this remark:—

"There are about fifty medical practitioners in Constantinople, principally Franks, from Italy and Malta, and a few Ionian Greeks, Armenians, and Copts; of this number there are, perhaps, five regularly educated physicians, and two of these are English gentlemen, highly respected, both by the Turks and Franks. Every *medico* has his allotted quarter; he beats this ground daily in pursuit of patients, and visits all the coffeehouses in the district with a Greek *drogueman*, as interpreter, at his heels, whose occupation it is to scent out sickness, and to extol the doctor. They are ever to be found on the most public bench of the coffeeshop, smoking with profound gravity, and prying into the features of those around them, for a symptom of disease. I confess I had to descend to this degradation, to get practice, in order to become acquainted with the domestic customs of the people. The first day my drogueman, who had just left the service of a Roman doctor, and had been practising on his own account since his discharge (for all

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drogueman become doctors), took upon him to teach me my professional duty, which he made to consist, in never giving advice before I got my fee, in never asking questions of the sick, and in never giving intelligible answers to the friends; I was to look for symptoms only in the pulse; I was to limit my *prognosis* to three words, '*In Shallah*,' or, 'Please the Lord,' for doubtful cases; and '*Allakharim*,' or 'God is great,' for desperate ones. I took my post in the coffeeshop, had my pipe and coffee, while my drogueman entered into conversation with the Turks about us." — Vol i. p. 54.

The story then proceeds as follows:—

"A well-dressed man, who had been sitting by my side, in silence, for half an hour, at last recollected he had a wife or two unwell, and very gravely asked 'what I would cure a sick woman for?' I inquired her malady,—'she was sick.' In what manner she was affected,—'why, she could not eat.' On these premises I was to undertake to cure a patient, who, for aught I knew, might be at that moment in articulo mortis. I could not bring myself to drive the bargain; so I left my enraged drogueman to go through that pleasing process. I heard him ask a hundred piastres, and heard him swear, by his father's head and his mother's soul, that I never took less: however, after nearly an hour's haggling, I saw fifty put into his hand; and the promise of a hundred more, when the patient got well, I saw treated with the contempt which, in point of fact, it deserved. No man makes larger promises than a Turk in sickness, and no man is so regardless of them in convalescence. I visited my patient, whom I afterwards found both old and ugly; but I was doomed, on the first occasion, to see no part of her form; she insisted on my ascertaining her disease with a door between us, she being in one room and I in another; the door was ajar, and her head, enveloped in a sheet, as it was occasionally projected to answer me, was the only part of her I had a glimpse of; this was the only woman I ever attended here or in the islands, who would not suffer the profanation of my fingers on her wrist. I, however, could just collect enough from the attendants, to cause me to *suspect* she had a cancer; and I did all, under such circumstances, that I could well do—I gave her an opiate."—pp. 57, 58.

At page 59 is a well told story of a Turkish consultation, at which the author assisted. Nothing can display more clearly the miserable condition of the medical interest in Turkey than this scene. A host of doctors, Jews, Greeks, Italians, and even Moslems, thronged round the sick man's bed. Amongst them were jumbled the friends, slaves, and followers of the poor patient. The latter gave their opinion as well as the doctors. But he who took the leading share in the business of the day, was a Turkish priest, who administered to the diseases

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both of soul and body. After a most unintelligible exordium, oil of wax was proposed, and agreed to. The doctors got their fee, (four Spanish dollars each, the only rational part of the story,) and the patient soon afterwards died. The secret of the Turkish priest's activity then came out. The bulk of the patient's property was invested in a mosque.

The faith of Turks in the power of amulets, fertilizing potions, and *madjouns*, seems to be universal. Indeed, from what we learn in these volumes, the principal business of the doctor is the prescribing of these *efficacious* remedies. "There are few Mahometans," says the author, page 63, "who do not put faith in amulets. I have found them on broken bones, on aching heads, and sometimes over love-sick hearts. The latter are worn by young ladies, and consist of a leaf or two of the hyacinth. Sometimes these amulets consist of unmeaning words, at other times of a scroll, bearing the words, 'Bismillah,' 'in the name of the most merciful God,' with some cabalistic sign; but most commonly they contain a verse of the Koran. In dangerous diseases recourse is had to the most potent of all charms, shreds of the clothing of the pilgrim-camel which conveys the Sultan's annual present to the sacred city. The amulet in most common use is an amber bead, with a triangular scroll worn over the forehead. This is probably an imitation of the phylacteries which the Jews were commanded 'to bind for a sign upon their hands, and to be as frontlets between their eyes.'" They are manufactured by Marabouts and Arab sheiks. Some very preposterous applications of a similar kind are occasionally to be seen, such as a roasted mouse laid upon a gun-shot wound, and intended to extract the ball. These absurdities, it may be said, only indicate the low state of intellect in the mass of the Turkish population, but it may reasonably be doubted whether the sick would be better off in the hands of the faculty.

Ladies were incessant in their demands upon the Doctor for some potion that would ensure fertility. A woman in Turkey has no honour or respect until she prove a mother, and all therefore are desirous of a progeny like Priam's. In spite of the specifics, however, they have in general but few children, for polygamy is undoubtedly injurious to population. But great as is the fondness of the women for medicines to make them fruitful, it is exceeded by that of the men for *aphrodisiacs*, which they denominate *madjoun*. The author was solicited for them in every province of the empire which he visited. It is lamentable to think that hardly a man arrives at the age of five and thirty whom debauchery has not debilitated and made

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dependent for his pleasures upon this sort of adventitious excitement. The common *madjoun* of Constantinople is composed of the pistils of the flower of the hemp plant ground to powder, and mixed up in honey, with cloves, nutmeg, and saffron.

Every one has heard of the opium-eaters in Turkey, and the author was naturally anxious to inform himself concerning this supposed fascinating practice. The coffee-houses where the *Theriakis*, or opium-eaters, assemble are situated in a large square, near the mosque of Solymania, and on a bench outside the door, they await the wished for reveries. There the author stationed himself to watch the effects of the potent drug. The gestures of the men were frightful. Those who were completely under the influence of opium talked incoherently. Their features were flushed, their eyes had an unnatural brilliancy, and the expression of their countenances was horribly wild. The effect is usually produced in two hours. The dose varies from three grains to sixty; one old man was seen by the author to take twenty-four grains in two hours. He had been in the habit of eating opium for five and twenty years.

The effects of this practice are painted by the author in the most dismal colours. "The debility," he says, "both moral and physical, attendant upon it, is terrible. The appetite is destroyed; every fibre in the body trembles; the nerves of the neck become affected, and the muscles get rigid," producing wry necks and contracted fingers. Life itself, as we may well suppose, is shortened by it. A regular opium-eater seldom lives beyond thirty years of age, if he commence the practice early. The habit, however, is too agreeable to be easily abandoned. The man is miserable till the hour arrives for taking his daily dose, but when its influence begins, he is all fire and animation. Some compose verses, and others harangue the bystanders, imagining themselves emperors, with all the harems in the world at their command.

The following detail of the author's own feelings when intoxicated by opium, is too curious to be omitted. It reminds us very strongly of the inhalation of the nitrous oxide which Sir H. Davy describes as producing a "thrilling, and a sense of tangible extension highly pleasing, in every joint." The dose which Mr. Madden took was four grains, shortly after which, he says—

"My spirits became sensibly excited: the pleasure of the sensation seemed to depend on a universal expansion of mind and matter. My faculties appeared enlarged: every thing I looked on seemed increased in volume; I had no longer the same pleasure when I

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closed my eyes which I had when they were open; it appeared to me as if it was only external objects, which were acted on by the imagination, and magnified into images of pleasure: in short, it was 'the faint exquisite music of a dream' in a waking moment. I made my way home as fast as possible, dreading, at every step, that I should commit some extravagance. In walking, I was hardly sensible of my feet touching the ground. It seemed as if I slid along the street, impelled by some invisible agent, and that my blood was composed of some etherial fluid, which rendered my body lighter than air. I got to bed the moment I reached home. The most extraordinary visions of delight filled my brain all night. In the morning I rose, pale and dispirited; my head ached; my body was so debilitated that I was obliged to remain on the sofa all the day, dearly paying for my first essay at opium-eating."—Vol. i., p. 26.

Early in the month of July, 1825, the author reached Alexandria, where the first thing that attracted his notice was the *climate* of Egypt. His observations on this very interesting topic are somewhat desultory, but we shall extract their substance for the benefit of our readers.

From the 1st of May to the 20th of June an easterly wind blows, called the kamsin, or simoom. It is the poison wind of the desert, and its effects on animal life are oppressive in the extreme. It produces such languor and exhaustion as made the author often lie for hours on his divan, incapable of the slightest mental or bodily exertion. The sensation was inexpressibly distressing. It was not, however, the degree of heat which occasioned it, for the thermometer is not affected more than five or six degrees during its prevalence. Perhaps some electrical condition of the air may be the real cause of this singularly depressing influence upon the nervous system.

The country, which has had no rain since March, is now completely parched up. The soil is split into innumerable cracks. The trees are scorched. The only plant that survives the drought is the alkaline salsola, which covers the burning sands. About St. John's day (the 24th of June) the face of nature changes. The north-west, or Etesian winds, begin to blow, and so continue till September, diffusing at Alexandria an agreeable freshness in the air. A heavy dew, called the *nocta*, falls also at this time. The drooping plants revive, and pestilence is stayed. Alexandria is at all times very damp. The atmosphere is saturated with a saline vapour, which condenses on the walls and furniture of the houses, in small crystals of nitre, sal ammoniac, and common salt. The soil is every where coated with these saline particles, and every thing made of iron rusts. Yet is this saline atmosphere not injurious

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to breathing: diseases of the lungs are unknown. Except during the prevalence of the Etesian gales, the sky of Egypt is serene and beautifully blue.

All Egypt in the vicinity of the river is a lake, from the beginning of August to the end of October; that is to say, the Nile then brings down all the moisture which the Etesian winds, loaded with clouds from the Mediterranean, had been carrying up since June. On the subsidence of the Nile, agriculture commences. Early in January spring puts forth its buds, and in April the first harvest is ended. By a system of irrigation the country is made to afford a second harvest, which is reaped in August, prior to the overflow.

At Alexandria the thermometer, during the summer months, seldom exceeds 90°, nor is the heat oppressive; yet, owing to other causes, its climate is the most unwholesome in all Egypt. The principal of these is the vicinity of the Lake Mareotis, now a saline swamp. The quarter of the city nearest the lake is subject to intermittent fevers in the spring, and to malignant putrid fevers in the autumn.

The climate of Upper Egypt is singularly dry, yet sycamores, five or six hundred years old, have thriven there without a drop of rain, and some, which are highly situated, without even deriving moisture from the inundation. A sheet of paper may be exposed there all night without its imbibing a particle of moisture, the *nocta* extending only to Lower and Middle Egypt. In Alexandria, Damietta, and Rosetta, there is more or less rain from November till March, and sometimes excessively cold weather; but in Cairo, though only one hundred and fifty miles distant, there is much less of both. In Upper Egypt there is no rain for six or even ten years, but when it does come, it is in torrents. During the intense heat of summer many birds leave Egypt, while the swallows of Europe make it their abode in winter. Their last starting-place appears to be the Morea.

A medical man travelling through Turkey must naturally hear much of the plague, but Mr. Madden did more: he saw a great deal of it, and studied the disease with a very proper degree of professional zeal, avoiding, we are happy to say, at the same time, those absurd attempts at bravado, which have cost some English physicians their lives, and others their character for common sense. One of the best chapters in Mr. Madden's first volume is that which he devotes to the subject of plague (Letter XVII. to Dr. Quin); and his opinions really appear to us so good, that it is but justice to bring them at

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some length before our readers. The author had some experience of the plague, both at Constantinople and in Candia, but his notions of it were then confused, sometimes believing it to be contagious, sometimes infectious, and sometimes neither the one nor the other. On his arrival at Alexandria he found the disorder very rife; the natives were perishing at the rate of eighteen per day, and few days passed over without the death of an European.

"For so small a population as that of Alexandria, say sixteen thousand souls, the mortality was considerable: every house was shut up, the servants were not suffered to go out, money was passed through vinegar before it was touched, letters were smoked, papers were handled with tongs, passengers in the streets poked unwary strangers with their sticks, to avoid communication, people thronged round the doctors' shops to know how many died in the night; the plague was discussed at breakfast, contagion was described at dinner, buboes and carbuncles (horresco referens!) were our themes at supper. The laws of infection were handled by young ladies in the drawing-room; 'a cat could communicate the plague, but a dog was less dangerous; an ass was a pestiferous animal, but a horse was non-contagious. Fresh bread was highly susceptible, but butchers' meat was non-productive.' If you looked at a man, he felt his groin; if you complained of a headach, there was a general flight; if you went abroad with a sallow cheek, the people fled in all directions; if you touched the skirt of a Christian's coat, you raised his *choler:* and if you talked of M'Lean, your intellect was suspected to be impaired."

The author visited the plague hospital daily, sometimes taking with him his host, Mr. Casey, whose fears he had somehow contrived to overcome.

"The pesthouse consists of several small rooms, with a grated window opposite the door facing the east, as if intended for receiving the poisonous wind of the desert. There is neither chair nor table in this dungeon; the sole furniture is a cane bed, called a *cafass*, with a mattress, and a sheet, which serves for a shroud a little later. The door is generally locked on the unhappy patient, an Arab attendant sits smoking his pipe outside, and very rarely enters to moisten the burning lips of the sufferer, or to lessen the terror of his solitary confinement; once a day the Italian doctor enters the room; orders a decoction of marshmallows, or elder-flower water, and then departs. Of all human horrors, earth has nothing to compare with the dismay depicted on the features of the sick, in these dreadful receptacles of pestilence!"

We would have wished to spare our readers a medical description of the plague, but the history which Mr. Madden gives of it, as occurring in his own servant, is so striking, and so illustra-

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tive of the common phenomena of the disease, that we cannot pass it over. He had taken his man with him to a supposed case of apoplexy; it proved to be the plague.

"The second day after this, I observed him staggering as he walked, his eyes had the expression of a drunken man's, his features were tumid, and yet he complained not; I asked him in the evening if he felt unwell? he said he had a cold; but I perceived he could hardly keep his feet: his pulse was very frequent, but easily compressed and not full, his tongue was of a whitish brown in the centre, with the borders very red.

"I saw the poor fellow had the plague, and I took him to the hospital. When we arrived there I saw him shudder (and well he might): he said to me, 'Don't you recollect, sir, I said in the *Bazaar*, that health is above every thing?' I never was more uncomfortable; I felt as if I was in some sort accessary to his disease. Headach and nausea distressed him from the time he was put to bed; he shivered frequently, but he said 'his heart was burning.' At night two livid spots were discovered on the forearm, with purple streaks, extending to the axilla and terminating in a bubo. His skin was parched and burning, his eye glaring on one object; and when his attention was called off, he talked incoherently, and complained of his tongue becoming swelled. His pulse at sunset was one hundred and eighteen, small and obstructed, his features swollen and of a sallow crimson hue; but next morning his colour was of a darker purple, such as denoted congestion somewhere, strangling the circulation. His regard was constantly fixed on the ceiling, and the low thick muttering of his lips had been incessant during the night. At four o'clock he bounced out of bed, escaped unnoticed, passed the outer door of the hospital, and ran, naked as he was, several yards in the direction of his home; but here he was overtaken by the people of the pesthouse; he had just sunk down quite exhausted. The strength of death, which had carried him thus far, was now gone; and, with the help of two Arabs, he was borne back to his dungeon, (for it deserved no better name,) trailing his feet, and his head sunk on his bosom. I saw him two hours after this; the bubo was the size of a small orange, the two livid spots had become large carbuncles, his eyes were glazed, yet unnaturally brilliant, and his fingers were playing with the bed clothes; at dusk the rattling of the throat was accompanied with spasms of the muscles of the neck; these went off, and after a couple of hours, without any apparent suffering, he died."

The author has his own speculations on the causes of plague, and upon the proper mode of managing it. These, we think, very rational and deserving of mature reflection. His notion is that the plague is essentially of *endemial* origin, in other words, that the original miasm is formed by some obscure putrefactive process, and that the atmosphere is only the *medium*

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by which the poisonous matter, thus eliminated, reaches the human body. He goes a step further, however, than this. Common malaria he believes to be formed from the decomposition *of vegetable* matter contained in the soil. Plague miasma, again, originates in the putrefaction of *animal* matter, the production of both depending on certain states of moisture and heat. But while the author is thus clear in attributing to plague an endemial origin, he is perfectly satisfied that it is also a contagious disorder, and that the contagious emanations from the bodies of the sick may produce the disease in others, in three different ways:—first, by contact; secondly, by means of the breath; and thirdly, by woollen clothes and other *fomites*, which have become saturated with contaminated air. The contagion of plague, according to Mr. Madden, requires to be in a certain state of *intensity* to produce the disorder in others. Hence it is, that with proper precaution, a pest hospital may be visited with impunity.

"In a word, plague under all circumstances is contagious, but under some, far more so than under others. In a well-ventilated chamber, where the bed-clothes are shifted daily, where the floor is washed daily, *and a fire kept constantly in the apartment* (*this I consider the most important agent of all in carrying off the foul air*) there is hardly any peril in approaching the bedside of the sick, avoiding his breath, and suffering no part of one's dress to touch the bedclothes. At four feet from the bed of the plague patient, in an airy room, there is no danger whatever. The miasma, I have ascertained, by much observation, (so far as an invisible agent is amenable to observation or experience) does not extend beyond a very few feet from its source; I would say, not four feet from the bedside, and then it becomes so diluted by the surrounding atmosphere as to prove innoxious."

From these statements it appears that the plague is, in the author's notion, more allied to typhus fever and to ague, than it is to small-pox and measles. It is held by the best physicians, that the two latter diseases are entirely the produce of *vital* actions, and that no combination of agents, exterior to the human frame, can give rise to them. The complete exemption of the world from these complaints for so many hundred years, and the fact that at St. Helena they are invariably imported, are decisive, we think, in favour of this doctrine.

On this point then we are perfectly agreed with the author. But we have our doubts how far he is right in attributing the origin of plague so exclusively to *animal* decomposition. He strives to account for it thus:—In Turkish towns the butchers kill their meat in the public streets. The streets are never

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cleansed." Dead dogs, cats, and rats, are constantly putrefying there. The carrion of camels and asses may be seen lying in the great thoroughfares. The Turks seldom change their linen, and in spite of their daily ablutions, are, in reality, a very dirty people. In every town of the Levant the Jewish quarter is the first affected by plague, and there every description of animal putrefaction is, par excellence, going forward.

We must do the author the justice to say, however, that he does not overlook the facts that seem to associate the plague with some condition of the soil. "It ceases," he tells us (p. 288), "when the inundation is established, and begins when the lands have been drained." This he attempts to explain by saying that the atmosphere is thereby rendered a better *recipient* of the pestilential effluvia which have their *origin* elsewhere; but we can hardly go along with him in this refinement. To all this theory, however, is appended the following very philosophical reflection:—

"I am endeavouring to illustrate this scourge of the Levant by facts, for I disclaim all theories. In a science, like that of medicine, where there are no general rules, there can be no unerring and universal principles; and, above all, in an anomalous disease, like that of plague, he who soars into the clouds to analyze the floating particles of miasma; to search after the causes of the *fomes*, and not to study its effects; to prove that the disease be infectious only, or contagious only; taken only by the breath, or only by the touch; to waste research and learning on mere terms; cavilling about distinctions between endemics and epidemics, but never turning attention to the treatment of the disease; that man, I say, may acquire notoriety, by the novelty or ingenuity of his theories, but he is not likely to lessen the mortality of the disorder."

The opinions which the author has been led to entertain on the treatment of the plague may be summed up in a few words. He condemns bleeding, and all measures of depletion, whilst he places the highest confidence in strong stimulants, diffusible and permanent. Wine and brandy were his sheet anchors. These he gave from the first moment the patient came under his care, even though the eye was suffused, the cheek flushed, and the skin dry. The first day he gave his brandy and water, one-third spirit; the second day he made it half and half; on the third day he contented himself generally with keeping up the excitement by strong Cyprus wine. If the patient live to the sixth day, he is very likely to recover. The third is that of greatest danger. By this treatment (with some other items of minor importance) he saved, in Candia, five patients out of nine. Every thing, however, he allows, depends on early

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treatment. So satisfied was he with his success at Candia, that on his arrival at Alexandria he proposed to attend plague patients for the season, and undertook to save from seventy to seventy-five per cent. of the sick. The measure, however, was never carried into effect; and we suspect, had the author tried his plan upon a large scale, he would have been disappointed. We are quite ready to admit that the principle of his treatment is good, but the virulence and depressing influence of the poison is such as to bid defiance to all ordinary restoratives. Besides, the plan has been tried and failed. In the Ionian Islands, in 1816, the tonic plan was pursued by several practitioners, but the patients died, in spite of wine, brandy, and opium.

At Cairo, Mr. Madden visited the lunatic asylum, and he favours us with some interesting observations on the state of eastern countries with regard to mental alienation. Fanaticism being a great source of insanity in most countries, and religious zeal being very strong in Turkey, one would think, *à priori*, that insanity should there be very frequent. The reverse, however, is the fact. There is very little madness in Turkey compared with other countries, which the author very reasonably attempts to account for in this manner. Turkish fanaticism is founded on certain essential doctrines of faith, which neither admit of doubt or disputation, whereas English fanaticism wants all this consoling security: "With us, the fanatic wavers with the wind of every doctrine; and while he works heaven and earth to gain his neighbour to his sect, his own bosom is distracted with a thousand doubts and scruples. His anxiety for his neighbour's soul undermines his own intellect at last; and thus fanaticism paves the road to Bedlam."

It is fortunate that insanity is rare in Turkey; for, judging from what the author saw at the lunatic asylum at Cairo, the poor creatures are miserably provided for. The *courbash*, a whip made of one solid thong of hippopotamus hide, was in constant use. When he inquired about their allowance, he heard, to his horror, that there was none except what charitable people were pleased to afford from day to day. The author, very kindly, sent for some food, which the poor creatures devoured like hungry tigers.

"There was one thing I could not help remarking. The ruling passion of the Mahometan character was preserved even in insanity. One man, who begged me to give him bread, spat upon me when he got it; another, who seized on the piece of water melon, which the women brought him, with all the eagerness of famine, abstained from eating it; hungry as he was, he preferred flinging it

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at a Christian's head rather than satisfy his craving stomach. He concealed it for near a quarter of an hour, till I was opposite his window, he then thrust his naked arm through the bars, and threw it in my face. In spite of my entreating, he got the *courbash* round his uncovered shoulders."

While travelling in Upper Egypt, the subject of embalming naturally came under the author's notice. He was a diligent investigator of the tombs with which that district abounds; and the following are a few among the interesting observations which his researches led to. The tombs are met with in the Libyan mountain, on the north-west side of Thebes. They perforate the mountain from top to bottom. The lowest are the most highly finished. These are inhabited by the Arabs, about three hundred of whom pass a miserable existence in these sepulchres of pride. The staple commodity of the place (Gourna) consists in mummies, the Arabs finding it easier to live by selling dead men than by the toil of husbandry. In the traffic of mummies, however, there appears to be no little portion of fraud; for the author states it as his firm belief, that in all the cabinets of Europe, there are not probably twenty mummies in the same coffin in which they were originally deposited. Having had the good fortune to cure one of the old troglodytes of a bad fever, he gained admission, with great difficulty, to the interior of the principal tomb, and there he found the manufacture of mummies going forward: that is to say, the best mummy cases being laid open, the original was taken out and sold, and its place supplied by one of an inferior kind. A little red paint in a coffee cup set all matters to rights again. From this he proceeded through a narrow passage into another cave, which was literally crammed with mummies. They were lying in horizontal layers, as they had, in all probability, been deposited some thousand years ago. In all the sepulchres which the author visited, he never found one mummy placed upright. Yet Herodotus so describes them. He purchased three mummies from his old friend, all in excellent preservation, for about sixteen shillings, the regular cost price for such articles from the Frank agents being from ten to fifteen pounds. They illustrated the three modes of embalming common among the Egyptians. The first consisted simply of drying. This could not have been practised generally in any other country than Upper Egypt, where the dryness of the air is so extraordinary. In Lower Egypt the mummies go to pieces on exposure to the external air; and at Alexandria, where the atmosphere is very humid, mummies, which had resisted corruption in a dry air for perhaps forty centuries,

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decomposed in as many hours. A few places in other parts of the world possess, from local causes, the same antiseptic property. The author mentions, as an instance, the vaults of St. Michael's church in Dublin.

The second mode of embalming consists in the injection of some antiseptic drugs previous to drying; and the third, which is the most perfect and sumptuous of all, is thus effected:—The viscera are removed, and the body sprinkled with aromatics and natron. After drying, it is enveloped in folds of gummed linen, and placed in coffins according to the condition of the deceased. The great principle of embalming is the exclusion of the external air, but much is undoubtedly attributable to the agency of antiseptics. The author ascertained that one of the principal ingredients in the mummy balsam was colocynth powder. The same drug is employed in Upper Egypt for destroying vermin in clothes, presses, and storerooms; and the ostrich-feathers sent to Lower Egypt are sprinkled with it. In the head of a mummy of a superior kind, he met with a balsam, in colour and transparency like a pink topaz. It burned with a beautiful clear flame, and emitted a very fragrant odour, in which the smell of cinnamon predominated. In the heart of one of the mummies he found about three drachms of pure nitre; the heart being entire, this must have been injected through the blood-vessels. Mummy powder was formerly in use all over Europe as a medicine, and, according to the author, is still employed as such by the Arabs, who mix it with butter, and esteem it a sovereign remedy for internal and external ulcers.

Another topic of inquiry suggested to the author by his residence in Upper Egypt, was the question, who are the descendants of the aboriginal mummified Egyptians? To decide this point, he made a collection of the skulls of the various inhabitants of Egypt,—Turks, Jews, Copts, Arabs, and Greeks, and the following are the conclusions to which he came. The old Egyptian head is of so peculiar a form, that it would be impossible to confound it with the Turkish, Grecian, or Arabic head. It is extremely narrow across the forehead, and of an oblong shape anteriorly. Among the many thousand mummy heads which he examined, he never found one with a broad expanded forehead. In phrenological language, those anterior organs which mark the seat of the reasoning powers were not well developed.

Niebuhr and most other travellers have stated the Copts to be the great body of the descendants of the Egyptians; but

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this the author will not agree to. The Coptic head is altogether of a different form. A line drawn across the orbits from one external angle of the eye to the other, is in the Copt half an inch longer than the same line of the mummy head. Herodotus describes the old Egyptians, among whom he was actually residing, as a people of black skins and short woolly hair. The Copts have neither the one nor the other. They were, in all probability, adds the author, a colony in Lower Egypt, in the time of the Egyptians, speaking their language, but not of their race.

"It is among the Nubians," says Mr. Madden, "that we must search for the true descendants of the Egyptians; a swarthy race, with wiry hair; surpassing, in the beauty of their slender forms, all the people of the East; living on the confines of Egypt, whither probably their ancestors had been driven by the Persians, and possessing a dialect which, though mixed with Arabic, no Arab understands." The measurement of the Nubian head corresponds with that of the mummy in every particular.

Having completed his survey of Egypt, the author prepared to visit Palestine. His journey across the Desert, tedious and painful as it was, afforded him the opportunity of making many interesting observations. These we must here endeavour to abridge.

Leaving San in company with his Bedouin guides, he started for Suez on a camel. The soil, for the first fifteen miles, (as far as Salehie,) was covered with a saline crust like hoar frost, which impeded vegetation, but did not altogether prevent it. The true sandy desert begins at Salehie, a string of miserable villages, with a population of about 8000 souls, shaded by a long row of date trees. A party of Bedouins, encamped in the neighbouring plains, received them kindly. A kamsin wind set in the following evening, attended with its usual debilitating effects. The sun was obscured with yellow clouds; the air was loaded with particles of sand; breathing became difficult. Sand was driving in furiously with the wind through every crevice in the tent, penetrating books and clothes, though tied up in a hair skin sack; it even got into the author's watch-case. The thermometer, at two o'clock, stood at 110° in the shade, and in the sand outside the tent, at 135°. The tent itself was like an oven. Starting at dawn next morning, our traveller soon lost all trace of vegetation; and he often wondered how, without landmark, trace in the sand, or compass, the Bedouins contrived to follow the proper route. Their whole study

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seemed to be to keep a straight course, occasionally looking back to observe their track, and to correct any little deviations. The *wadys* or wells where they took up their stations for the night, afforded some bad water. The dew which then fell was heavy. The Bedouin maxims for preserving health in the Desert are highly extolled by the author. They are, never to drink in the day-time, nor to sleep with the head uncovered. The more a traveller drinks during the day, the more thirsty he gets; at night, he may drink to his heart's content. The Bedouins seem to follow the example of their camels, and lay in overnight a stock of water for the next day's journey. The author is half inclined to attach some value to the Arab notion of a morbific influence in the moon. Ophthalmia and catarrh are especially considered to be owing to moonbeams. "Strange as this may seem," says the author, "I really believe there is some influence more than that of common dampness in the nights here." He was strangely perplexed with that singular phenomenon of the Desert, the *mirage*; but this we must allow him to describe in his own animated language.

"We had now journeyed in the Wilderness three days without meeting a human being, and without seeing any living creature. With all my endeavours to resist the delusion of the *Mirage*, I found it quite impossible this day to persuade myself that my senses did not deceive me. At one moment, the rippled surface of a lake was before my eyes; at another time, a thick plantation appeared on either side of me; the waving of the branches was to be seen, and this view was only changed for that of a distant glimpse of a city: the mosques and minarets were distinct, and several times I asked my Bedouins if that were not *Suez* before us; but they laughed at me, and said it was all sand; and what appeared to me a city, a forest, or a lake, the nearer I endeavoured to approach it the farther it seemed to recede, till at last it vanished altogether, 'like the baseless fabric of a vision, leaving not a wreck behind.' If I were to speak of the nature of the *Mirage* from my own sensations, I should say, it was more a mental hallucination than a deception of the sight; for, although I was aware of the existence of the Mirage, I could not prevail on myself to believe that the images which were painted on my retina were only reflected, like those in a dream, from the imagination; and yet so it was." Vol. ii. pp. 199, 200.

The theory of the formation of a sandy desert occupied the author's thoughts. Whence came the accumulation of sand? Did it always exist there, and occupy the same extent of surface? or can its origin be traced to depopulation and the want of cultivation? The sight of the wide ocean of the Wilderness

naturally suggested these questions, but their solution, says Mr. Madden, is far from being easy. He scouts Dessaix's notion that Nature, having expended all her art in perfecting the rest of the world, left the Desert but half made up; and throws out the following for want of a better explanation:—

"The Deserts, I imagine, from the peculiarity of their situation, were the last places from which the waters of the Deluge retired; consequently the deposition of sand, in those places, was much greater than elsewhere. This sand is identical with that of the ocean; it is formed of the same transparent particles of quartz and silex. In all probability, in ancient times, it did not occupy the tenth part of the surface which it now does; but when population diminished and cultivation ceased, the sands in the interior were dispersed by the prevailing winds, particularly those of the north and west, over the plains; and the soil, for want of irrigation, became an arid surface: plantation, which above all impedes the accumulation of sand beyond it, when no longer attended to, favoured the desolation of the land.

"On the seacoast, particularly of Egypt, the flatness of the country allows a free passage to the winds, which come loaded from the shore with particles of sand. Thus I particularly remarked on the shores of Rosetta and Damietta, near the Boghas, the setting up of a small stick on the shore would be a sufficient *nucleus*, in the course of a few months, for the formation of a mountain of sand. One thing is certain, that wherever there is water, no matter in what part of the Wilderness, there vegetation is to be found. The stopping up of canals, and the want of irrigation, are the great causes of desolation which favour the extension of the Desert. The country from San to Salehie, and probably to Suez, was formerly a cultivated country: the ruins of palaces, such as those of Zoan and that of the *Beit Pharoon*, now in the middle of the Desert, prove that the country around them must have been cultivated, and that, at a very short period before our era."

The latter half of the second volume is occupied with some very interesting pictures of the Holy Land. We can only find room, however, for the following sketch of the Dead Sea, or the Sea of Lot, as the natives call it. From the summit of a sterile rock, he first looked down upon the glossy lake, three hundred feet below him. The towering mountain on the opposite coast coast appeared almost ten miles distant.

"The moon was shining in all her oriental splendour, on the desecrated scene; the shadows of the rugged promontories around me were reflected on the lake; but on its surface not a ripple was to be seen; the silence of death was there, and the malediction of heaven was written on the soil! For miles around me there was

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life in neither air, earth, nor water. I sickened of the prospect; my spirits were completely overpowered.

"I reposed on the bare rock for half an hour; my feet were cut in many places with the sharp flints which abound here, and it was with difficulty I could descend the mountain. About six in the morning I reached the shore, and much against the advice of my excellent guide, I resolved on having a bath. I was desirous of ascertaining the truth of the assertion, that 'nothing sinks in the Dead Sea.' I swam a considerable distance from the shore; and about four yards from the beach I was beyond my depth; the water was the coldest I ever felt, and the taste of it most detestable; it was that of a solution of nitre, mixed with an infusion of quassia. Its buoyancy I found to be far greater than that of any sea I ever swam in, not excepting the Euxine, which is extremely salt. I could lie like a log of wood on the surface, without stirring hand or foot, as long as I chose; but with a good deal of exertion I could just dive sufficiently deep to cover all my body, but I was again thrown on the surface, in spite of my endeavours to descend lower. On coming out, the wounds in my feet pained me excessively: the poisonous quality of the waters irritated the abraded skin, and ultimately made an ulcer of every wound, which confined me fifteen days in Jerusalem; and became so troublesome in Alexandria, that my medical attendant was apprehensive of gangrene."

On the shores of the lake the author found several fresh-water shells, and the putrid remains of two small fish, which he believes to have been carried down by the Jordan, for he is convinced that no living creature is to be found in the Dead Sea. He spent two hours in fishing, but he only caught some bitumen. The face of the mountains and of the surrounding country bore to him all the appearance of a volcanic region, though he confesses he neither found pumice-stone nor genuine black lava. The soil was covered with white porous stone and red-veined quartz. On the mountains on the western side of the lake were large quantities of the stink stone, the recent fracture of which produces a strong smell of sulphuretted hydrogen. The surface of the water on these shores is covered with a thin pellicle of inflammable asphaltum. This proceeds from fissures in the rock on the opposite beach. After coagulating in the cold air, it cracks in pieces with an explosion, and is drifted over to the western beach. On coming out of the water the author found his body coated with it, and likewise with an incrustation of salt, about the thickness of a sixpence. The rugged aspect of the mountains, the terrible ravines on either shore, the romantic forms of the jagged rocks, all prove that the surrounding country has been the scene of some terrible convulsion of nature. I have no hesi-

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tation, adds the author, in saying, that the sea which occupies the sites of Sodom and Gomorrah, Adam, Seboim, and Segor, covers the crater of a volcano; and that, in all probability, heaven made that mode of destruction the instrument of Divine vengeance. A bottle of the water of the Dead Sea, which Mr. Madden brought home with him, was analyzed last winter by Dr. William Gregory, at the London University. The following is his analysis:—

Chloride of sodium, with a trace of bromine	9.59
Chloride of magnesium	5.28
Chloride of calcium	3.05
Sulphate of lime	1.34
	19.26

The most extraordinary circumstance perhaps to be remarked is, that there is no visible outlet to the lake, notwithstanding that the Jordan is continually flowing into it. Dr. Shaw calculates, that the Jordan daily sends into the Dead Sea six millions and ninety thousand tons of water, and yet there is never any visible increase or diminution in the height of the water, though Chateaubriand erroneously states that it varies at different periods. Its greatest breadth does not exceed ten miles, and its extreme length is about seventy."

With this we must conclude our extracts from Mr. Madden's very interesting volumes. We had marked many other passages for notice, for the author was very observant, and many curious facts of a scientific nature will be found dispersed through his pages. But we have said enough we think, to attract the reader's attention to the work, and to impress him with a favourable idea of Mr. Madden's talents. One of the great merits of the book is its adaptation to so great a variety of tastes. The physician, the divine, the politician, and the mere lover of adventure, will find in it wherewithal to interest him; and the sketch of the work now given will, at least, suffice to show, that it is not unworthy the careful perusal of the man of science.

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Further Recommendations respecting the Use of Lights in the Cornish Fisheries.

Dear Sir,

I attempted some time ago, and through a former number of your Journal, to call the attention of the proprietors of the Cornish fisheries to the probable advantages that might be derived from the use of lights, as practised in so many parts of the world, towards attracting the shoals of the pilchard, or inducing them to come nearer to the shore than it is conceived they have done for some years past. That this notice did not fail of its intended effect, is at the same time a proof of the influence of your Journal, and an inducement to renew the same subject in this manner, rather than through the common and vulgar method of a newspaper. But it proves what is of much more consequence, the activity of mind of that really enlightened and watchful people: since it is rare to find improvements, even when established, instead of suggested, as in this case, adopted by any class of persons accustomed to a routine, and since no other fishermen throughout England or Scotland have thought fit to make the same experiment.

Such conduct is an encouragement towards bringing this subject once more under the public notice; and, if I am not misinformed as to the management of these trials in Cornwall, to which, if rightly represented to me, their failure may probably be traced, a few further remarks on the same subject cannot be misplaced, and may possibly lead at some future time to better success.

As I understand, (I hope the report was correct,) the method alone that was adopted was to make lights on shore, hoping that by this method the shoals which might possibly be swimming far off might be enticed toward the land, where alone, and in very shallow soundings, this, which is a seine fishery, can be conducted. If no other mode was attempted, perhaps I ought to take part of the blame to myself, since the remarks made on their tendency towards the Eddystone lighthouse, a tendency which, as to all fish, will be found to hold almost universally where lighthouses exist, might have seemed to indicate that to imitate this by means of a light on shore would have been

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sufficient. Let me be permitted, therefore, to suggest what I consider more likely to be successful, premising also a few remarks, which are, however, unfortunately little more than hints and conjectures.

I have shewn on a former occasion, in your Journal, that the changes of the herring, as to place, are even more capricious than those of the pilchard have ever been, while in some cases also their abandonment of particular shores has proved particularly durable. Nor is this less true of all fishes, though most conspicuous in the gregarious ones. The cause would concern us more, could we find the remedy; though we cannot, it is still an object of rational curiosity. And it probably consists in previous change of place, or deficiency in their natural food, though of this we can equally give no account, and have only removed the difficulty from one set of animals to another. That the occasional disappearance or diminution of gregarious fishes, perhaps in particular, is also sometimes dependant on epidemic, or *epizotic* disease, I have suggested to be probable in another work, but cannot enlarge on it here.

Now, as to the actual fact of the variable presence or abundance of those endless and multitudinous marine animals which are probably the food of various fishes, there can be no doubt, although naturalists, like fishermen, have seemed to pay no attention to it; nay, not even to the existence of such animals; and after all their voyages and studies, have utterly overlooked, not ninety-nine in a hundred, but far more, even of those which swarm about our own shores, to say nothing of the vast ocean through all parts of the world, which they have traversed on these very pursuits. My own experience is narrow, but it is at least sufficient to establish the point. And the general result, as it bears on this question, is, that having discovered nearly two hundred undescribed species, rather by accident than design, since it was not my pursuit, in the short space of six weeks, and with a very few hours of those weeks, on a very limited tract of coast; and further found that in such places the whole sea was almost a mass of life, it cannot but follow that, consistently with the universal order of nature, these are the very food of the other tribes which exceed them in magnitude, and that here, probably, in particular, the great armies of the grega-

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rious fishes, such as the pilchard and herring, find their food. To suppose that they do not eat, as fishermen imagine, because food is not found in their stomachs, would be an anomaly in the laws of nature, that no sound physiologist can admit.

Nor, indeed, is that presumed fact fairly stated. If the stomachs of these fishes are widely examined they will not be found empty, though we cannot detect organized forms in them, as we find entire crabs in the stomach of a cod-fish. Nor is this surprising, when we consider how small and how tender the tribes of marine worms and insects are, and how rapid is the digestive power of fishes. And, to come more nearly to the point in question, I have had further occasion to observe, during various summers in the same seas, that while some shores abounded in such animals, such food, others were entirely destitute, and not only so, but that in one summer, during two entire months, scarcely a single animal was to be found, not one medusa, for example; while, in a previous or subsequent one, the seas were alive with them.

Here then is, or may be, a cause, or the cause, of the recent absence of the pilchard from the Cornish coasts, or of its comparative absence, that absence concerning this fishery, immediately, as it relates to the shallow soundings near the shore, But whatever the cause be, if their only change is to have quitted those soundings for deeper water, as has been more than once said, it is by the project in question that it is proposed to recal, or entice, and circumvent them; should they have entirely abandoned the coast or channel, or should the race be absolutely diminished, there can be no hopes. But if they do revisit the coast still, however distantly, a fact which can be ascertained, I must think that Cornwall will not show its usual acute attention to commerce or industry, if it does not persevere in these efforts, and in a more efficient manner. The contingent gain so often experienced is tempting, and it is moreover true that a very extensive capital is lying dormant, or producing actual and annual loss.

They may be reminded here, that the places of the fish during the time of the trials which they made might have been such as to render the lights invisible, or so distant as to render those inefficacious; nor, speculating on this influence as expe-

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rienced in other fisheries, should I consider that a light on the shore would engage their attention, unless they were already in such depths as to have permitted the use of the seine. Hence, then, I would suggest to the fishermen the adoption of the mode actually practised in the Mediterranean and the American rivers, or elsewhere; and this is, to carry their lights to sea, or to establish a sufficient fire-boat as the leader, and as an essential part of the arrangements for a seine. It is not necessary to say how this ought to be constructed; it is sufficiently obvious, since its essence is a grate with flaming fuel, so as to maintain a large, brilliant, and durable light. Nor need I suggest to these active and keen fishermen that their object should be to attempt, first, through scouts, to discover where the fish are, and then by means of the light to entice them to follow into the requisite sounding. It is not easy to believe that it would be inefficacious, nor to admit this proposal to be termed a wild speculation, because it would be a very singular anomaly that this fish alone, of all those on which the experiment has ever been tried, should be uninfluenced by that which influences the whole race, apparently because to the whole, as I have shewn when explaining the use of the universal luminous property of fishes, it is the indication of the presence of their food, their general guide to their prime object, through the darkness of the night or the deep ocean.

Thus have I taken the liberty to urge this question once more on the proprietors of these fisheries, regretting only that I cannot hope to be a witness, or to assist more usefully, and making the only necessary apology if I have here proceeded on wrong information. I have yet also, however, to learn, why the seine should be the exclusive inst[illegible]t of the pilchard fishery, why the *driving* or herring ne[illegible]ot be applied to the one case as well as the other, if [illegible] are determined to hold to the deep water. The captu[illegible]t not be so gre[illegible] but the necessary capital is less; and [illegible]erring fishery i[illegible] profitable trade, why would not the [illegible] one be the sa[illegible] under the same system? [illegible]en the [illegible]e of the fish[illegible] the oil are, I believe gre[illegible]rely [illegible]qual to the[illegible] herring.

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On Cookery in general, and on the Works of Ude and Jarrin in particular.

We are induced to particularise the works of Messrs. Ude and Jarrin, because they form the most complete "Code of Cookery" ever presented for the world's edification; and also because we are convinced that the well-being of the nation depends more on that useful science, SCIENCE as we are now bound to call it, than the public in general are aware of.

It might, perhaps, be going rather too far, to charge the atrocities of the French Revolution to those "*Artistes*," who left their ordinary duties to turn orators, or to say that the cruelties of Robespierre and Carnot, with the other monsters who acted in the national tragedy, arose from the crudities of undressed viands; and yet, when it is recollected that all the useful arts were done away, and cookery amongst the rest was so unnaturally misused to be applied to the purposes of state, when the country was *embroiled*,—the "First Etat," in a *ferment*,—the Noblesse in *hot-water*, and M. de Colonne making a *hash* of affairs; we repeat, that when ministers thus usurped the *professor's* chair, (his stew-pan we should rather say,) and by their inability the state *cauldron* was *boiling over*, it was no wonder that domestic cookery should fall into disrepute. In fact, the "Chef de Cuisine" was superseded by the "Chef d'Armée;" as was the "Batterie de la Cuisine" by the "Batterie de la Guerre."

This state of affairs is feelingly alluded to and explained by M. Ude. "The Goddess of Reason employed no "Maitre d'Hotel." The Parisian dealers in "*Epice Fine*" were as cruelly treated as

> Poor Barto-Vallé, and melancholy Burgess;
> Victims of Pitt, of Huskinson, and Sturges.

Our author, Ude, begins his useful book with an elaborate history of French cookery, headed by a Greek quotation, which we at first sight took to be μεγα κακον, and that, by a free translation, being made "great cake," it might have reference to pastry and pie-crust; but on closer inspection we found it to be μεγα βουλ, and other Greek words, implying, "great

mouth, great understanding." Under which is a more intelligible but homely observation of the great lexicographer, Johnson, "that he who does not mind his belly will hardly mind any thing else."

French cookery seems to have been in a very inferior state, up to the time of the Reformation, when the illustrious Luther made mastication, as well as morals, an object of consideration: ancient dogmas on dishes were done away, and freedom of eating and thinking came into fashion together; the enjoyments of feasting succeeded the severities of fasting; and the great Gonthier appeared to raise the culinary edifice, as Descartes raised that of philosophy.

The English reader, whose excursions in culinary affairs have not extended beyond the pages of that primitive old lady, Mrs. Glasse, may never have heard of d'Alégre, Souvent, Richant, and Mézelier, but it is not to be believed that there can be any one unacquainted with the "great Gonthier!" he who overturned *bromatological* traditions, and established the *nervous glands* as the sovereign judges of the table!—"who in less than ten years invented seven culisses, nine ragoûts, thirty-one sauces, and twenty-one soups!"—We cannot, we repeat, for one moment believe that there can be an individual who is not familiar with the discoveries of the great Gonthier! We recollect that the discovery of the fricandeau alone was the glory of the reign of Henri de Valois, and though "*its discovery*," as Mons. Ude justly observes, "*appears not more wonderful* than that of America," yet it required, nevertheless, "*une grande force de tête*."

We know nothing, however, of the latter days of this cooking Copernicus, and whether he was roasted or broiled for conforming to the Reformation, or died a natural death, is now matter of conjecture. The pedantic historians, who have circumstantially described the foolish disputes of the sixteenth century, calculated to shorten the duration of human life, are silent about him, whose labours went to its extension, and who gave action to a system, which, he trusted, might prevent the world from returning to its original chaos.

During the reigns of Louis XII. and XIV. cookery made great strides, and what Lorenzo di Medici did for learning and

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letters in Italy, his daughter Catherine did for cookery and carving in France.

This princess, the mother of kings, endowed with the gifts of fortune, and by nature with the gift of a palate, whose intuitive sensibility was never equalled, crossed the Alps with a noble troop of cooks and conjurers. By the aid of Berini, she recovered from oblivion *sauces* which had been lost for ages. "Let the foolish vulgar," says our enlightened author, "laugh at the importance which the Queen-mother placed in the art of cookery:"—"it was at the table she named her deputies, bought votes, and softened the rigid virtue of a *modern Cato*."

The long reign of this celebrated woman was distinguished by the splendour of her repasts, though the glory of some of them was tarnished by the purposes for which they were given. One was for the arrangement of the massacre of St. Bartholomew; and another, in honour of the execution of Cavages, whom her son Charles wished to see hanged.

Some uncandid historians have insinuated, that the cooks of this tyrannical lady were, according to the expression of Tacitus, *instrumenta reginæ*, ministers of her vengeance, and instruments of her cruelty. At this Mons. Ude is very indignant; and he very slyly insinuates, that she had doctors and apothecaries, whose heads were more fertile in inventions than that of a devoted artist, who never left his kitchen.

What was thus so happily commenced by Catherine, was most vigorously carried on, and improved, by her hungry son, Henry III., who spent whole days at the table. The constellations of the kitchen shone, as might be supposed, with the greatest splendour, under this gourmand king. He had the good fortune to witness the invention of the *fricandeau*—an invention, considered only second to the wheel-barrow and spinning-wheel!

We cannot follow our author through all the mazes of monarchical influence over cookery, and the consequent national prosperity. The nation continued to flourish during the reign of Henry III.; and we had always entertained a high respect for Henry IV.; but, if the poor have preserved his memory, his cooks have not. He did nothing for them; and they, in return, did nothing for him. A fatal love for romantic gluttony

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threatened destruction to the progress of the art. English pudding and beefsteaks found their way into France, and, but for the providential interference of Mezelier, Richant, Robert, and other great names, which all cooks should for ever revere, her sons would have fed on dishes hardly worthy the early ages of the Roman republic.

As the troubles which preceded the Revolution advanced, cookery receded, and the dreadful scenes of that bloody drama so bewildered the cooks, that it would appear the executive government had found it necessary to turn cooks themselves, and intolerably bad ones they were;—but, "fortunately, the eighth of Brumaire pulled down the five Directors, together with their saucepans."

In the scramble that took place, Bonaparte picked up the crown, and Talleyrand the stew-pan. It was the Prince de Talleyrand who rekindled the sacred flame in France. The first clouds of smoke, which announced the resurrection of the science of cookery in the capital, appeared from the kitchen of an ancient bishop. "History will never forget that great man, who aspired to all kinds of glory, and would have been, if he had wished, as great a cook as he was a statesman."

The garlic and onions of the republicans began now to give way to the *piquantes* beauties of Beauvilliers and Balaine. Grisnod de la Reynière produced the *Almanach des Gourmands*, and the joyous disciples of Epicurus hailed, with exultation, the revival of gastric science.

The alteration this grand event effected in the manners and language of Europe is hardly to be conceived. Gastronomy arose—and, with all the refinements of a fine art, produced a race unknown before,—eaters and drinkers, with taste and genius. To this were all the nobler qualities of the mind applied:—Gastronomy became a fashionable word in the fashionable world; and a dandy, who would have fainted at such a subject in former times, could now discourse on it as if inspired!

"But what is this gastronomy?" said a country cousin to us one day,—"what is this gastronomy we hear so much about; I cannot find it in Johnson?" To have answered, that is was like Scotch broth, or soup maigre, would have settled the

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query with some; but not so with our intelligent querist. Let us take the definition from the French, with whom it originated: "La gastronomie est la connaisance raisonnée de tout ce qui a rapport à l'homme, en tout ce qu'il se nourrit." Such is the importance given to this art in France, that it is proposed to erect an "*Academie des Gastronomes.*" The minister who accomplishes this will be, amongst ministers, what Henry IV. was amongst kings, "et son éloge sera dans toutes les *bouches*."

Taste used to be a metaphor to express that judgment, which each man formed to himself, of things not contained in any rules, or admitting of demonstration. Since gastronomy arose, however, this metaphorical sense of the word seems to be lost sight of; and it is now only used to express the sensation of the palate, or, according to Johnson's definition, "it is the act of tasting"—"gustation." A French professeur has written two volumes of Scientific Meditations on "Phisiologie du Gout;" and taking this view of it, says, "Le goût, est celui de nos sens, qui nous met en relation avec les corps *Sapides*, au moyen de la sensation qu'ils causent dans l'organe destiné à les apprécier."

The right of judging and tasting for one's self, which is a natural privilege of man, seems in this age to have been surrendered to M. Ude, and no man can now expect to be received into good society, who does not eat according to the laws of gastronomy. He would be a bold man who should venture to avow his love for good old English fare, or who would not give up eating what he likes, to appear to understand what he does not like:

"Not for himself he sees, or hears, or eats,
"*Artists* must chuse his pictures, music, meats."—Pope.

The frugal meal was formerly the time for unbending the mind, by cheerful and improving conversation, and the "Table Talk" of Selden and others has been thought worthy of being transmitted to posterity. But now, gastronomy occupies the whole of our thoughts and conversation; our attention is directed to the critical examination of compound dishes, and should any interesting subject arise, it is put an end to by the ecstatic interjections of "excellent! exquisite! delicious! pray taste this! pray taste that!"

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The dispensation of garlic is a matter of the nicest judgment; garlic in the hands of the cook is like prussic acid in the hands of the doctor, and becomes poison or panacea, according to the skill with which it is administered. The learning displayed on this subject is the test of gastronomic profundity. In some dishes there should be a mere "*soupçon*," in others "*un petit bran*." In bechamele, for instance, it should be the former. The comfort and harmony of a fashionable party was very nearly destroyed, a short time ago, by the garlic being "*un peu trop forte*." My Lord having tasted and duly considered the bechamele, shook his head, and gave it as his opinion, that the garlic was not enough concealed, but earnestly begged the company to taste it with attention, and give their sentiments upon it.

The company, after proper deliberation, confirmed his Lordship's opinion, that "the garlic did indeed distinguish itself too much!" The artist was condemned, the dish was d—d; when the Maître d'Hotel put the matter in a different position, by stating, that the cook had only the night before received an official intimation, "*that they were now stronger than ever in garlic at Paris*." On which the company, one and all, said, "*That* altered the case."

This ingenious and edifying controversy is continued sometimes through four courses, till the company can neither swallow nor utter any more.

Nor are these schisms in sauces confined to the gustatory organs alone; the organ of vision must be gratified. The eye, "critically nice," as Sir Joshua used to remark, regards the *tints*. "What did you think of our dinner yesterday?" said an antiquarian friend of ours, to the late Greek-gourmand of Soho: "Was it not excellent? was it not well cooked?"—"Yes, it was; every thing was well done—but—" "But what?" "It was *a little too brown!*"

A very great person of ancient times was asked, if he was not ashamed to play so well upon the fiddle? May we not, with as much reason, ask these illustrious moderns, are you not ashamed of being such good cooks? Yet such is the rage amongst our higher orders, at present, that we expect to see a "Digest of the Noble Art of Cookery," by "Persons of Qua-

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lity,"* whose philosophy, now-a-days, seems very similar to that of Shadwell's Virtuoso, who, when asked, "Whether life does not consist of the four elements *?" answers, as an alderman, "Faith, so they say; but, I think, it rather consists of eating and drinking."

Now eating, in a philosophical view, ought rather to be a subject of humiliation than pride, since the imperfection of our nature appears, in the daily necessity we are under of recruiting by it. Hence, we should conclude, that the object of a rational being should be, to repair his fabric as substantially and as cheaply as possible, which was the notion of our forefathers.

Eating, in truth, like all other customs, is national, and it has been said of the inhabitants of this isle, that they fight as they eat; and we cannot but admire a laconic harangue a colonel in the Guards once made to his grenadiers. Just before the company were to advance to a desperate attack, a general officer began to excite them with a fine speech. "What signifies all this," cries the colonel; "remember, my *English* lads, your *beef* and *pudding;* march up and singe the beards of them!—March."

Much learned authority might be quoted to show that the battles of Agincourt, Poictiers, Ramillies, and Blenheim, were won per force of British beef. We will illustrate this point by a circumstance which occurred in the polite warfare of those times. During a truce the cook of a marshal of France invited the Duke of Marlborough's cook to dine with him; the Frenchman had at his entertainment all the extraordinary dishes the fertile imagination of his country's art could invent, or his own genius produce. The Englishman allowed him to be a perfect master of his profession, and a day was fixed for returning the visit. The appointed day, the cook and the guests arrived, and when all were in expectation of a masterstroke, in giving some dishes a false appearance, or in the scientific seasoning of others, there was brought in a plain *sirloin of beef*, with a plum-pudding. After a shock of surprise, "Sir," says the Frenchman, "this is so uncommon a dish on

* A ridicule on the medical theory, which supposed *health* to consist in the just balance of the elements in the human body.

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this occasion, that I did not expect any thing like it." "Very likely, Monsieur," replied Mr. Bull; "but this is a dish for every Englishman to be proud of; this dish has carried my countrymen twice through France already, and I don't doubt but it will a third time." Mr. Bull was not far off in his prophecy, as history records.

Of the antiquity of the culinary art there is no doubt. Athenæus affirms that the first kings of the earth were cooks! The Patriarchs were the same. The art is, in fact, as old as King Cadmus; Patroclus was famous for his Olla Podrida; and a Roman general received the Samnite ambassadors in the room where he was boiling turnips for his dinner; but who would wish to dine with such fellows? "Bœuf seignant, mouton bélant, porc pouri: tout n'en vaut rein s'il n'est bien cuit."

To make a just comparison between ancient and modern systems, a few specimens of each should be set beside each other. To do this let us go back in the history of the art to anno 1630. We find Master John Murrel complaining of the ignorance of his fraternity; and in a book dedicated most appropriately to the daughter of the Lord Mayor, confessing that "Cookerie-books instruct how to marre, rather than make, good meate," and then most profoundly and profitably correcting former errors, he sets forth the most approved methods, with the newest art of carving and serving, together with an exact order for making *kickshawes*. Kickshawes! what would the Greek artist of Crockford's say to a delicacy made of minced bullock's kidney! what to a "rack of veal," or a "farced leg of mutton!" or "an umble pie!" (failing of umbles, made of lamb's head and purtenance;) or "a chewit of stockfish!" or a "Fryday's pie," without either flesh or fish! or a fashionable cabinet dinner in the days we speak of, consisting of a dish of marrow-bones, a leg of mutton, a dish of fowls, three pullets, and a dozen larks, all in the same dish! But this was the fare in the dark ages of alimentation, when, as Master Murrel shrewdly observes, cookery was likely "to marre, rather than make good meate."

Cookery, in this country, had been greatly neglected, and it was not till some four physicians thought the subject worthy of their consideration, that public attention was directed to

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what is now considered a most important science. It is very true, Sir Theodore Mayerne and Dr. Lister had both written "de Opsoniis;" but it was Sir John Hill who composed the grand code of Mrs. Glasse, which edified our grandmothers, and Dr. Hunter who enlightened the world with "Culina," to whom we are indebted for first introducing what Montaigne designates "la science de la gueule." But even now, if we believe our author, we are in a barbarous state of simplicity: "Quel étrange pays! vingt religions, et qu'une sauce! Melted butter and anchovies, melted butter and capers, melted butter and parsley, melted butter and eggs, and melted butter for ever!"

Guy Patin said that the French hated the English "parce qu'ils versoient du beurre fondu sur leur veau rôti,"—and here we find Mr. Ude's national antipathy breaking forth. His sneer at melted butter and the Lord Mayor's coach is an affront to the Court of Aldermen. He is almost in as great a rage as the French gentleman, who, when some melted butter was spilt on his silk coat, exclaimed, "Sacre! these English feed on nothing but blood and butter!" Very different was the mode in which Lord John Townshend received a similar salute. He was accidentally at dinner with an intimate friend, where, amongst other servants, the coachman waited at table. In lifting a butter-boat, honest John spilt a considerable part of its contents over his lordship's clothes; whilst they were assiduously wiping it off, "John," said he, with great good nature, "you should never *grease* any thing but your wheels."

We must now notice the chapter which treats "on the imputed ill effects of cookery on the health;" by far the most *piquante* part of the work. Here he dips deep into the "stock-pot" of knowledge; quotes Galen and Hippocrates, makes a *fiery* attack on the doctors, and we think is rather *saucy* to the ladies, whom he considers as the most powerful antagonists to cookery. The doctors, from time immemorial, have been declared enemies of his class; but let the College look about them, Ude has put on his war-boots, and threatens to emancipate mankind from the despotic dominion of medicine. "Fuge omnes medicos, atque omnimoda medicamenta."—"Venite ad me, qui stomacho laboratis, et ego restaurabo

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vos!" He not only declares war against the doctors, but means himself to practise as one; and there are many professors of the art to be found, he says, "of proper information, and sufficiently devoted to the interests of the human race, to give prescriptions in cookery, as doctors do in medicine." Nor does he omit to mention the advantage that a skilfully-dressed dish has over the "doctor's chocolate." He does not favour us with a prescription; but, for a sick alderman, we can suppose, from what he says, it would be something after this style:—

℞ Soupi Turtulorum, ℔iss.
Turboti, ℔i.
Vensoni Hanchici, ℔ij.
Tarti *Rhubarb* } partes equales.
Puddeni Plumbor. }
Adde Vin. opt. Madeir. vel Oport. quant. suf.

Who would send to *Paris* for a prescription, when they could get one from Ude? Who would get rhubarb from the chemist in preference to that of the cook? Who would not send such a prescription to Peacock's City Dispensary in Bishopsgate, rather than to Apothecary's Hall?

That the kitchen is the handmaid to physic is no new doctrine: the learned Donatus thought so centuries ago, and the chemical preparations with which modern cookery abounds have materially strengthened the alliance; it is the glory of skilful cookery to render all alimentary substances salubrious—from pork to pistachio-nuts. In the probable course of events, therefore, there will soon be no diseases to cure; cookery, like vaccination, will lessen the evils of Pandora's box;—the cook will supersede the doctor—and the cookery-book that of domestic medicine.

Not long ago, a friend of ours calling on a sick alderman, found him deeply engaged in reading. After the usual compliments, he said, "Studying a medical work, I presume?" "Indeed no," replied Mr. Alderman; "I have got Mrs. Glasse's Cookery, to try if I can find any thing I can eat; and see, in the last page, here is a 'cure for a surfeit,' which will suit me exactly, for that, to tell you the truth, is my complaint!"

Before we conclude this part of our subject, we must enter our protest against certain barbarous measures resorted to, even

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in these refined times, which are on a par with roasting and braizing live swallows, whipping live pigs, and scarifying live fish; we mean the oleaginous dropsy, or unnatural obesity which

" ——— makes modern meat
Too dear to buy, too fat to eat;"

and particularly the liver complaints, produced in that stupid yet amiable bird, the goose. To render him worthy of being placed in a pâté, " *il faut sacrifier la personne de la bête!*" which is nailing him by the foot close to a large fire, where it is confessed that he passes " *une vie assez malheureuse*"—which he could not support but from the consolation of the great honour that awaits him—" cette perspective lui fait supporter sex maux avec courage; et lorsque son foie, *plus gros qu'elle même*, et lardé de truffes, revêtu d'une pâte savante, ira par l'entremise de M. Corcellet porter dans toute l'Europe la gloire de son nom, elle se résigne à la destinée, et ne laisse pas même couler une larme!" Nor do the more respected gallinagenous tribe escape these horrible practices, who suffer by mutilation and cramming, for the same exalted purposes, giving them an embonpoint which nature never intended.

We must also express our regret that a man of so much erudition and refinement did not extend his system a little further, and give a code of " Rules for the Decorums of the Table." The great Dilworth, at the end of his Spelling-book, condescended to instruct, " à la Chesterfield," little boys and girls in their general behaviour.

How well might our ingenious author have enlarged, after this mode, on the best and most graceful style of handling a silver fork, shewing the national peculiarities in the use of this modern appurtenance of the table:—how an Englishman places it by the left side of his plate, and uses it in conjunction with the knife:—how the Frenchman takes his fork in one hand, and a piece of bread in the other;—how the German, being bothered with it, holds it perpendicularly to his plate;—while the Russian contents himself with using it as a toothpick! Then what a theme for a Greek cook would have been that unceremonious and uncourteous proceeding called *bolting!* in all its different varieties.

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Our author has not only "la force de tête" to make a sauce, but the genius of poetry to describe it.

"A sauce, made according to the principles of the art, excites and restores the appetite, flatters the palate, is pleasing to the smell, and inebriates all the senses with delight." What can be more poetic? Sauces are to food what action is to oratory; and "the prodigy of a perfectly made sauce" is, we know not exactly why, considered amongst the proofs of the immateriality of the soul! But his prose often assumes the loftiness of poetry, and the "Metamorphoses of Ovid" are outdone by our author, who informs us that during the time of the Regent of France, when spices were banished by the prevalence of sugar, "all the gods of the ancients came down from Olympus upon tables in the *form of pies*."—Preface xlv.

Like all great masters of arts, he is very unmerciful to pretenders. Quin thought it would be a public benefit to hang a cook or two; "One skeleton of a cook," said he, "would save us." Dr. Johnson threatened to throw a rascally cook into the river; Wenceslaus, Emperor of Germany, *roasted his cook alive*, for a little peccadillo; and our great master has such a horror of the quacks in his learned profession, that he is ever making them feel his raillery "piquante."

It must nevertheless be confessed, that Ude has done more to uphold the "*nobility and dignity of his art*" than any of his predecessors; and sincerely do we hope that he may long continue to practise, for the well doing of his own animal machinery, and the comfort and health of all those who have the good fortune, and are privileged, to partake of his *entrées*, by having the *entrée* to "Crockford's House." He is, in good truth, no ordinary person! As Pope said of Akenside, "he is no every day writer,"—"il parle grec comme Homère,"—the *pen*, and the *pan*, are wielded by him like the spear of Diomed, *παλαμαι τε παλαμαις*;—we do not apologise for quoting ancient Greek, considering that the subject in discussion is written by a learned person, who sets us the example, and who, by his intercourse with *modern Greeks*, is probably equally well acquainted with their dialect.

In our youthful days, we remember to have read of Marshal *Turene* (or Turenne), and have a faint recollection that he

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was a warrior; but if, on the contrary, he should prove to have been an ennobled cook, we must say, with such an historical precedent, to let our scientific author remain "*au naturel*," like an ungarnished dish, would be a scandal to the age we live in, and though we do not venture to interfere with Sir George Nayler, or the Herald's College, we humbly hint our persuasion, that the Emperor of Elba, in his best day, would have been proud to bestow on him an order of merit, and he would have blazed in his title page, as Prince Perigord—Duke de Ragoût—Baron Bechamele!

We shall resume this important subject at an early opportunity.

Catalogue of the Works of Sir H. Davy, *of* Dr. Wollaston, *and of* Dr. Young.

Titles of Papers by Sir H. Davy, *in the Transactions of the Royal Society.*

An Account of some Galvanic Combinations formed by Single Metallic Plates and Fluids, analogous to the Galvanic Apparatus of M. Volta. Read June 18, 1801. Vol. xci., p. 397.

An Account of some Experiments and Observations on the Constituent Parts of certain Astringent Vegetables; and on their Operation in Tanning. Read Feb. 24, 1803. Vol. xciii., p. 233.

An Account of some Analytical Experiments on a Mineral Production from Devonshire, consisting principally of Alumine and Water. Read Feb. 28, 1805. Vol. xcv., p. 155.

On a Method of Analysing Stones containing a Fixed Alkali, by means of the Boracic Acid. Read May 16, 1805. Vol. xcv., p. 231.

The Bakerian Lecture. On some Chemical Agencies of Electricity. Read Nov. 20, 1806. Vol. xcvii., p. 1.

The Bakerian Lecture. On some New Phenomena of Chemical Changes produced by Electricity, particularly the Decomposition of the Fixed Alkalies, and the Exhibition of the New Substances which constitute their Bases; and on the General Nature of Alkaline Bodies. Read Nov. 19, 1807. Vol. xcviii., p. 1.

Electro-Chemical Researches on the Decomposition of the Earths; with Observations on the Metals obtained from the Alkaline Earths; and on the Amalgam procured from Ammonia. Read June 30, 1808. Vol. xcviii., p. 333.

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The Bakerian Lecture. An account of some new Analytical Researches on the Nature of certain Bodies, particularly the Alkalies, Phosphorus, Sulphur, Carbonaceous Matter, and the Acids hitherto undecompounded; with some general Observations on Chemical Theory. Read Dec. 15, 1808. Vol. xcix., p. 39.

New Analytical Researches, on the Nature of Certain Bodies, being an Appendix to the Bakerian Lecture for 1808. Vol. xcix., p. 450.

The Bakerian Lecture for 1809. On some New Electro-Chemical Researches, on various objects, particularly the Metallic Bodies from the Alkalies and Earths; and on some Combinations of Hydrogen. Read Nov. 16, 1809. Vol. c., p. 16.

Researches on the Oxymuriatic Acid, its Nature, and Combinations, and on the Elements of Muriatic Acid; with some Experiments on Sulphur and Phosphorus, made in the Laboratory of the Royal Institution. Read July 12, 1810. Vol. c., p. 231.

The Bakerian Lecture. On some of the Combinations of Oxymuriatic Gas and Oxygen, and on the Chemical Relations of these Principles to Inflammable Bodies. Read Nov. 15, 1810. Vol ci., p. 1.

On a Combination of Oxymuriatic Gas and Oxygen Gas. Read Feb. 21, 1811. Vol. ci., p. 155.

On some Combinations of Phosphorus and Sulphur, and on some other Subjects of Chemical Inquiry. Read June 18, 1812. Vol. cii., p. 405.

On a new Detonating Compound, in a Letter from Sir H. Davy to Sir Joseph Banks. Read Nov. 5, 1812. Vol. ciii., p. 1.

Some Further Observations on a new Detonating Substance. Read July 1, 1813. Vol. ciii., p. 242.

Some Experiments and Observations on the Substances produced in different Chemical Processes on Fluor Spar. Read July 8, 1813. Vol. ciii., p. 263.

An Account of some new Experiments on the Fluoric Compounds; with some Observations on other Objects of Chemical Inquiry. Read Feb. 13, 1814. Vol. civ., p. 62.

Some Experiments and Observations on a new Substance which becomes a Violet-coloured Gas by Heat. Read Jan. 20, 1814. Vol. civ., p. 74.

Further Experiments and Observations on Iodine. Read June 16, 1814. Vol. civ., p 487.

Some Experiments on the Combustion of the Diamond, and other Carbonaceous Substances. Read June 23, 1814. Vol. civ., p. 557.

Some Experiments and Observations on the Colours used in Painting by the Ancients. Read Feb. 23, 1815. Vol. cv., p. 97.

Some Experiments on a Solid Compound of Iodine and Oxygen, and on its Chemical Agencies. Read April 20, 1815. Vol. cv., p. 203.

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On the Action of Acids upon the Salts, usually called Hyper-Oxymuriates, and on the Gases produced from them. Read May 4, 1815. Vol. cv., p. 214.

On the Fire Damp of Coal Mines, and on Methods of Lighting the Mine so as to prevent its Explosion. Read Nov. 19, 1815. Vol. cvi., p. 1.

An Account of an Invention for giving Light in Explosive Mixtures of Fire Damp in Coal Mines, by consuming the Fire Damp. Read Jan. 11, 1816. Vol. cvi., p. 23.

Further Experiments on the Combustion of Explosive Mixtures, confined by Wire Gauze, with some Observations on Flame. Read Jan. 25, 1816 Vol. cvi., p. 115.

Some Researches on Flame. Read Jan. 16, 1817. Vol. cvii., p. 45.

Some new Experiments and Observations on the Combustion of Gaseous Mixtures, with an Account of a Method of Preserving a continued Light in Mixtures of Inflammable Gases and Air, without Flame. Read Jan. 23, 1817. Vol. cvii., p. 77.

On the fallacy of Experiments in which Water is said to have been formed by the Decomposition of Chlorine. Read Feb. 12, 1818. Vol. cviii., p. 169.

New Experiments on some of the Combinations of Phosphorus. Read April 9, 1818. Vol. cviii., p. 316.

Some Observations on the Formation of Mists in particular Situations. Read Feb. 25, 1819. Vol. cix., p. 123.

On the Magnetic Phenomena produced by Electricity. Read Nov. 16, 1820. Vol. cxi., p. 7.

Some Observations and Experiments on the Papyri found in the Ruins of Herculaneum. Read March 15, 1821. Vol. cxi., p. 191.

Further Researches on the Magnetic Phenomena produced by Electricity; with some new Experiments on the Properties of Electrified Bodies, in their relations to Conducting Powers and Temperature. Read July 5, 1821. Vol. cxi., p. 425.

On the Electrical Phenomena exhibited in Vacuo. Read Dec. 20, 1821. Phil. Trans. 1822, p. 64.

On the State of Water and Aëriform Matter in Cavities found in certain Crystals. Read June 13, 1822. Phil. Trans. 1822, p. 367.

On a New Phenomenon of Electro-Magnetism. Read March 6, 1823. Phil. Trans. 1823, p. 153.

On the Application of Liquids formed by the Condensation of Gases, as Mechanical Agents. Read April 17, 1823. Phil. Trans. 1823, p. 199. (Appendix to the preceding paper.)

On the Changes of Volume produced in Gases in different States of Density by Heat. Read May 1, 1823. Phil. Trans. 1823, p. 204.

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On the Corrosion of Copper Sheathing by Sea-water, and on Methods for Preventing this Effect; and on their Application to Ships of War and other Ships. Read Jan. 24, 1824. Phil. Trans. 1824, p. 151.

Additional Experiments and Observations on the Application of Electrical Combinations to the Preservation of the Copper Sheathing of Ships, and to other purposes. Read June 17, 1824. Phil. Trans. 1824, p. 243.

Further Researches on the Preservation of Metals by Electro-Chemical Means. Read June 9, 1825. Phil. Trans. 1825, p. 328.

The Bakerian Lecture. On the Relations of Electrical and Chemical Changes. Read June 8, 1826. Phil. Trans. 1826, p. 383.

On the Phenomena of Volcanoes. Read March 20, 1828. Phil. Trans. 1828, p. 241.

Account of some Experiments on the Torpedo. Read Nov. 20, 1828. Phil. Trans. 1829, p. 15.

Sir H. Davy also published the following works:—

Researches Chemical and Philosophical, chiefly concerning Nitrous Oxide. 8vo. 1800.

Syllabus of a Course of Lectures on Chemistry. 8vo. 1802.

A Lecture on the Objects of the Royal Institution. 8vo. 1810.

Elements of Chemical Philosophy. Vol. i. 8vo. 1812.

Elements of Agricultural Chemistry. 4to. 1813.

On the Safety Lamp, and on Flame. 8vo. 1828.

Discourses to the Royal Society. 4to. 1828.

Salmonia, or Days of Fly-fishing. 12mo. 1828.

List of Papers printed in the Transactions of the Royal Society, by W. H. Wollaston, M.D.

On Gouty and Urinous concretions. Read June 22, 1797. Vol. lxxxvii., p. 386.

On Double Images caused by Atmospherical Refraction. Read March 6, 1800. Vol. xc., p. 239.

Experiments on the Chemical Production and Agency of Electricity. Read June 25, 1801. Vol. xci., p. 427.

A Method of examining Refractive and Dispersive Powers, by Prismatic Reflection. Read June 24, 1802. Vol. xcii., p. 365.

On the Oblique Refraction of Iceland Crystal. Read June 24, 1802. Vol. xcii., p. 381.

The Bakerian Lecture. Observations on the Quantity of Horizontal Refraction; with a Method of Measuring the Dip at Sea. Read Nov. 11, 1802. Vol. xciii., p. 1.

On a new Metal found in Crude Platina. Read June 24, 1804. Vol. xciv., p. 419.

On the Discovery of Palladium: with Observations on other Substances found with Platina. Read July 4, 1805. Vol. xcv. p. 316.

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The Bakerian Lecture. On the Force of Percussion. Read Nov. 14, 1805. Vol. xcvi., p. 13.

On Fairy Rings. Read March 12, 1807. Vol. xcvii., p. 133.

On Super-acid and Sub-acid Salts. Read Jan. 28, 1808. Vol. xcviii., p. 96.

On Platina and Native Palladium from Brazil. Read March 22, 1809. Vol. xcix., p. 189.

On the Identity of Columbium and Tantalium. Read June 8, 1809. Vol. xcix., p. 246.

Description of a Reflective Goniometer. Read June 8, 1809. Vol. xcix., p. 253.

Croonian Lecture. On Muscular Action, Sea Sickness, and the salutary Effects of Exercise on Gestation. Read Nov. 16, 1809. Vol. c., p. 1.

On Cystic Oxide, a new species of Urinary Calculus. Read July 5, 1810. Vol. c., p. 223.

On the Non-existence of Sugar in the Blood of Persons labouring under Diabetes Mellitus. Read Jan. 24, 1811. Vol. ci., p. 96.

On the Primitive Crystals of Carbonate of Lime, Bitter-spar, and Iron-spar. Read Feb. 13, 1812. Vol. cii., p. 159.

On a Periscopic Camera-Obscura and Microscope. Read June 11, 1812. Vol. cii., p. 370.

The Bakerian Lecture. On the Elementary Particles of certain Crystals. Read Nov. 26, 1812. Vol. ciii., p. 51.

On a Method of Freezing at a Distance. Read Dec. 17, 1812. Vol. ciii., p. 71.

A Method of Drawing extremely fine Wires. Read Feb. 18, 1813. Vol. ciii., p. 114.

Description of a Single-Lens Micrometer. Read Feb. 25, 1813. Vol. ciii., p. 119.

A Synoptic Scale of Chemical Equivalents. Read Nov. 4, 1813. Vol. civ., p. 1.

On the cutting Diamond. Read May 2, 1816. Vol. cvi., p. 265.

Observations and Experiments on the Mass of native Iron found in Brazil. Read May 16, 1816. Vol. cvi., p. 281.

On the method of cutting Rock Crystal for Micrometers. Read Feb. 24, 1820. Vol. cx., p. 126.

On Sounds inaudible by certain Ears. Read June 29, 1820. Vol. cx., p. 306.

On the Concentric Adjustment of a Triple Object-Glass. Read Dec. 13, 1821. Phil. Trans. 1822, p. 32.

On the Finite Extent of the Atmosphere. Read Jan. 17, 1822. Phil. Trans. 1822, p. 89.

On Metallic Titanium. Read Dec. 12, 1822. Phil. Trans. 1823, p. 17.

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On the Apparent Magnetism of Metallic Titanium. Read June 19, 1823. Phil. Trans. 1823, p. 400.

On the Semi-decussation of the Optic Nerves. Read Feb. 19, 1824. Phil. Trans. 1824, page 222.

On the Apparent Direction of Eyes in a Portrait. Read May 27, 1824. Phil. Trans. 1824, p. 247.

The Bakerian Lecture. On a Method of rendering Platina malleable. Read Nov. 20, 1828. Phil. Trans. 1829, p. 1.

A Description of a Microscopic Doublet. Read Nov. 27, 1828. Phil. Trans. 1829, p. 9.

On a Method of comparing the Light of the Sun with that of the Fixed Stars. Read Dec. 11, 1828. Phil. Trans. 1829, p. 19.

On the Water of the Mediterranean. Read Dec. 18. 1828. Phil. Trans. 1829, p. 29.

On a Differential Barometer. Read Feb. 5, 1829. Phil. Trans. 1829, p. 133.

A Catalogue of the Works and Essays of the late Dr. Young. *Found in his own Hand Writing, to* 1827.

A Short Note on Gum Ladanum, with a verbal Criticism on Longinus, signed with his initials, and inserted in the Monthly Review for 1791, seems to have been his first appearance before the Public. The criticism was admitted by Dr. Burney to be correct.

In the Gentleman's Magazine for April 1792, Observations on the Manufacture of Iron: an attempt to remove some objections to Dr. Crawford's Theory of Heat, which had been advanced by Dr. Beddoes.

Entomological Remarks: Gentleman's Magazine, December 1792: on the habits of Spiders; on a passage of Aristotle, with an illustration of the Fabrician System; and a plate of the mouth of an insect.

Observations on Vision: Philosophical Transactions, 1793, p. 169, explaining the accommodation of the Eye, from a muscular power in the crystalline lens—a theory not altogether new, but immediately afterwards claimed by John Hunter, as a discovery of his own.

Contributions to Hodgkin's Calligraphia Græca, 4to. London, 1794; including Lear's Curses in Iambics.

Description of an Opercularia. Linnæan Transactions, vol. iii. p. 30. London, 1797: read in 1794. The Opercularia Aspera of Gaertner, called by Persoon, Cryptospermum Youngii, from the name here suggested.

Some Notes and an Epigram, in Dalzel's Collectanea Græca, 8vo. Edinburgh, 1795.

De Corporis Humani viribus conservatricibus, Dissertatio, 8vo. Gottingen, 1796: an Inaugural Dissertation, collected from a multiplicity of authors.

Translation of Lichtenstein on the Genus Mantis. Linnæan Transactions, vol. vi. p. 1. Read in 1797.

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. The Leptologist. British Magazine, 1800; a Series of Essays on Grammar, Criticism, Geometry, Paintings, Manners, Riches, Exercises, Medicine, and Music; some of them reprinted afterwards.

There is also an account of the French Calendar and Measures, and an Essay on the Morals of the Germans.

Experiments and Enquiries respecting Sound and Light. Philosophical Transactions, 1800, p. 106: the vibrations of the air observed by means of smoke; those of strings counted, and their orbits observed with a microscope; their harmonics suppressed at pleasure.

A Bakerian Lecture on the Mechanism of the Eye. Philosophical Transactions, 1801, p. 23: describing a new Optometer, and showing that the eye retains its power of accommodation under water; measuring also the dispersive power of the eye. (Dr. Y. remarks, that he "afterwards found that his own eye lost almost the whole of its power of accommodation soon after fifty, remaining fixed at its greatest focal distance."

A Letter respecting Sound and Light. Nicholson's Journal, August 1801, in answer to Professor Robinson, of Edinburgh.

A Syllabus of a Course of Lectures on Natural and Experimental Philosophy; 8vo. London, 1802: presenting a Mathematical Demonstration of the most important Theorems in Mechanics and in Optics; and containing the first publication of the general law of the INTERFERENCE OF LIGHT, which has been considered as the happiest result of all the Author's efforts. It was not till the year 1827, that the importance of this law could be said to be fully admitted in England: it was in that year that the Council of the Royal Society adjudged Count Rumford's Medal to M. Fresnel, for having applied it, with some modifications, to the most intricate phenomena of polarized light.

A Bakerian Lecture on the Theory of Light and Colours; Phil. Trans. 1802, p. 12, developing the Law of Interference, and entering into all the details of the theory to which it leads; dwelling, at the same time, upon the difficult points, with somewhat more of candour than might have been consistent with his object, had he been anxious to obtain proselytes.

An Account of some Cases of the Production of Colours, p. 387, containing a simpler statement of some applications of the same law, intended to exhibit the facts in a more concentrated form.

A Reply to Mr. Gough's Remarks. Nicholson, November, 1802, p. 1. This Letter, together with some subsequent Correspondence, relates principally to the coalescence or composition of Sounds, affording an analogy to the interference of Light.

Journals of the Royal Institution, 8vo. London, 1802-3. A first volume, and part of a second were edited, and chiefly written, by Dr. Young.

Experiments and Calculations relative to Physical Optics. Phil. Trans. 1804, p. 1. Another Bakerian Lecture, continuing the demonstration and the application of the Law of Interference.

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A reply to the Animadversions of the Edinburgh Reviewers, 8vo. 1804: —A defence of the Papers printed in the Transactions against two Articles supposed to have been written by Mr. Brougham.

To an Imperial Review, which was an unsuccessful speculation of some booksellers in 1804, he contributed several medical and some other miscellaneous Articles. The works that he reviewed were, *Dumas Phisiologie*, Darwin's Temple of Nature, Blackburn on Scarlet Fever, Percival's Medical Ethics, Fothergill's Tic Douloureux, Crichton's Table, Nisbet's Watering Places, Rowley on Madness, *Hutton's Ozanum*, Buchan on Sea-Bathing, *Robison's Astronomy*, Winterbottom's Sierra Leone, Macgregor's Medical Sketches, Wilson's Philosophy of Physic, Richerand's Physiology, and Joyce's Scientific Dialogues.

An Essay on the Cohesion of Fluids. Phil. Trans. 1805, p 71, containing many of the results, which were published as new, about a year afterwards, by La Place. The mathematical reasoning, for want of mathematical symbols, was not understood, even by tolerable mathematicians; from a dislike of the affectation of algebraical formality, which he had observed in some foreign authors, he was led into something like an affectation of simplicity, which was equally inconvenient to a scientific reader.

A Course of Lectures on Natural Philosophy and the Mechanical Arts; two volumes, 4to. London, 1807. This elaborate work was the result of the unremitting application of five years; two, whilst the Author was engaged in giving Lectures at the Royal Institution, and three more in compiling the mass of references contained in the second volume, and in incorporating their results, when requisite, with the text of the first. By means of numerous plates, and by indexes of various kinds, he had endeavoured to render the book as convenient for occasional reference, as it was correct for the purposes of methodical study. (The failure of the booksellers who published this work, at the moment of its appearance, so greatly injured its sale at the time, that it did not repay the expenses of the publication; and Dr. Young considered that his labours were first generally appreciated by the Natural Philosophers of the Continent.)

Remarks on Looming, or Horizontal Refraction. Nicholson, July, 1807, p. 153, supplying some deficiencies in Dr. Wollaston's Theory, particularly with regard to the occurrence of actual Reflection.

A Table of Chances, with Remarks on Waves. Nicholson, Oct. 1807, p. 116.

A Theory of Covered Ways and Arches. Nicholson, Dec. 1807, p. 24.

Remarks on a Pamphlet of Professor Vince. Nicholson, April, 1808, p. 304; pointing out the mathematical fallacy of the Professor's supposed refutation of the hypothesis of Newton, respecting the cause of Gravitation.

Calculation of the Rate of Expansion of a Supposed Lunar Atmosphere. Nicholson, June, 1808, p. 117.

Determination of the Figure of a Gravitating Body. Nicholson, June, 1808, p. 208.

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Calculation of the Attraction of a Spheroid. Nicholson, August, 1808, p. 273.

A Review of Sinclair on Longevity. British Critic.

Abstracts and Criticisms in "The Retrospect," about 1808 and 1809.

Hydraulic Investigations. Phil. Trans. 1808, p. 164; principally subservient to an intended Croonian Lecture.

A Syllabus of a Course of Lectures on the Elements of the Medical Sciences; 8vo. London, 1809. These Lectures were delivered for two seasons at the Middlesex Hospital. (Dr. Young remarks, that "they were little frequented, on account of the usual miscalculation of the Lecturer, who gave his audience more information in a given time, than it was in their power to follow.")

Computation of the Depression of Mercury in the Barometer. Nicholson, March, 1809, p. 215. A Continuation of the Paper on the Cohesion of Fluids, Oct. p. 81.

Remarks on the Friction of Wheels, in Buchanan's Essay on Wheelwork, 8vo. Glasgow, 1809.

A Croonian Lecture on the Heart and Arteries. Phil. Trans. 1809, p. 1; attempting to demonstrate, on Mathematical principles, that the larger arteries can have little or no concern in propelling the blood by their active muscular powers.

A Numerical Table of Elective Attractions. Phil. Trans. 1809, p. 148; with remarks on the sequences of double decompositions, shewing that if numerical expressions of Electric attractions are possible, their effects in double decompositions may be compendiously expressed by tables of sequences only.

A Memoria Technica for Elective Attractions, in a few Latin hexameters. Nicholson, April, 1809.

Account of the Pharmacopeia Londinensis, in Cumberland's London Review, 1810.

To the earlier Volumes of the *Quarterly Review*, he contributed a variety of Articles, which frequently, according to the custom of modern times, contained more of original research than of immediate criticism. To Vol. i. La Place, Action Capillaire. Vol. ii. Haslam, Pinel, Cox, and Arnold, on Insanity; La Place, Refraction Extraordinaire. Vol. iii. *Herculanensia;* Jones on the Gout; *Memoires d'Arcueil.* Vol. vi. Cuthbert on the Tides. Vol. viii. Davy's Chemical Philosophy. Vol. ix. Blackall on Dropsies. Vol. x. Adelung's Mithridates; Göthe on Colours. Vol. xi. *Malus, Biot, Seebeck, and Brewster on Light;* Bancroft on Dyeing; Davy's Agricultural Chemistry; Adams on Ectropium. Vol. xiii. *Wells on Dew.* Vol. xiv. Jamieson and Townsend on Languages; Pym and Fellowes on Yellow Fever, an Article printed, but not published in the Work. Vol. xix. p. 411; *Restoration and Translation of the Inscription on the Sphynx.*

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Berzelius on Definite Proportions, from the German, appeared in several successive Numbers of the Philosophical Magazine, from January 1813 to April 1814.

A Theory of the Tides. Nicholson, July, Aug. 1813.

An Introduction to Medical Literature, including a System of Practical Nosology, 8vo. London, 1813: a work of considerable labour, though far less arduous than the "Natural Philosophy." The Appendix contains an Abstract of Berzelius's Animal Chemistry, from the Swedish. To a second Edition, published in 1823, were added the References to later Journals, and an Essay on Palpitations, which first appeared in the fifth Volume of the Medical Transactions of the College of London.

Remarks on the Employment of Oblique Riders, and on other Alterations in the Construction of Ships. Phil. Trans. 1814, p. 303; the substance of a Report, before presented to the Board of Admiralty, relating to Sir Robert Sepping's Improvements, with some additional illustrations.

An Investigation of the Thrust of Soft Substances. Hutton's Mathematical Dictionary, Edition 2. 1815. Article Pressure.

A Practical and Historical Treatise on Consumptive Diseases, 8vo. London, 1815; being a condensed abstract of every thing recorded to have been said or done, with regard to Consumption. Particular circumstances had pressed the Publication of this Work within nine months after it had been commenced.

In the eighteenth Volume of the Archæologia, London, 1815, appeared some *Remarks on Egyptian Papyri*, and on the Inscription of Rosetta, annexed to a Communication made by Sir William Edward Rouse Boughton, Bart. They contain an interpretation of the principal parts of both the Egyptian Inscriptions on the Pillar found at Rosetta, and consequently a Key to the lost Literature of Ancient Egypt; though, for professional reasons, the discovery was made public with as little parade as possible.

Extracts of Letters and Papers relating to the Egyptian Inscription of Rosetta, in the Museum Criticum of Cambridge, Part vi. 8vo. 1815; a Correspondence with MM. Silvestre de Sacy and Akerblad.

An Investigation of the Pressure sustained by the fixed supports of Flexible Substances. Phil. Mag. Sept. 1813, applied to the Hoops of Casks, and to Dock Gates.

An Algebraical Expression of the Values of Lives. Phil. Mag. Jan. 1816, with a Diagram.

Account of some Thebaic Manuscripts, written on Leather. Legh's Narrative, 4to. London, 1816.

Additional Letters relating to the Inscription of Rosetta; the first addressed to the Archduke John, who had lately been in England; the second to M. Akerblad. Museum Criticum, Part vii. *The Letters were printed and distributed in* 1816; *the Journal was not published till* 1821. They

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ANNOUNCE THE DISCOVERY OF THE RELATION BETWEEN THE DIFFERENT KINDS OF EGYPTIAN LETTERS OR CHARACTERS—the basis on which the system of M. Champollion was afterwards erected.

Letter of Canova, and two Memoirs of Visconti, translated from the French and Italian, 8vo. London, 1816. A Volume of 200 pages, which was completed in twelve days; together with remarks on an error of Delambre, which was afterwards confuted more at large by Mr. Cadell.

It was in 1816, that Dr. Young complied with an application made to him by Mr. M'Vey Napier, to write some articles for a Supplement to the Encyclopædia Britannica, conducted under the superintendence of that gentleman, and completed in 1825.

The articles which he furnished were: Atwood—Addendum to Annuities—Bathing—G. Beccaria—Bloch—Borda—Boulton—Bramah—BRIDGE—Brisson—Bryant—Camus—*Notes* on Carpentry—Cavallo—*Cavendish*—*Chromatics*—Cohesion—Condamine—Coulamb—Dollond—Dolomieu—Duhamel—EGYPT—Fermat—Fluents—F. Fontana—G. Fontana—J. R. Forster—J. G. A. Forster—Fourcroy—Frisi—Guyton de Morveau—Herculaneum—Hydraulics—Ingenhousz—Lagrange—La Lande—Lambert—LANGUAGES—Lemmonier—Luc—Malus—Maskelyne—Mason—Mechain—Messier—Orme—Pallas—Pauw—PORSON—Preservers of Life—Road-making—*Robison*—Rush—*Steam-Engine*—Tennant—Thomson Count Rumford—TIDES—Tooke—Wakefield—Watson—Weights and Measures—Polarization by Arago, translated, with *Notes*. In all, about sixty-three articles, each marked with two different letters. (These were two consecutive letters of the sentence "*Fortunam ex aliis*;" the *u* in *fortunam* being sometimes printed as a *v*.)

Remarks on some Theorems relating to the Pendulum. Phil. Trans. 1818, p. 95, in a Letter to Captain Kater.

Translation of some Greek Inscriptions. Light's Travels. 4to. London, 1818.

Specimen of a Greek Manuscript in the possession of the Earl of Mountnorris, 1819. Archæologia, Vol. xix. This may possibly have been a pawnbroker's account: another piece nearly resembling it was sent by Mr. Salt to the British Museum.

Remarks on the Probabilities of Error in Physical Experiments, and on the Density of the Earth, considered especially with regard to the reduction of Experiments on the Pendulum, Phil. Trans. 1819, p. 70; computing the density of the earth, upon the supposition of the compression of a homogeneous elastic substance only.

Dr. Young edited the Nautical Almanac, from the year 1819, for the remainder of his life.

Remarks on Laplace's latest Computation of the Density and Figure of the Earth. Quarterly Journal, April, 1820; determining the Ellipticity, on the supposition of a compressed elastic substance.

Dr. Young furnished quarterly, for many years, to the Journal of Science

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and Arts, about twenty pages of Astronomical and Nautical Collections, beginning in 1820; the greater part either original or translated by himself.

Appendix to the second edition of Belzoni's Travels, 4to. London, 1821.

Elementary Illustration of the Celestial Mechanics of Laplace, 8vo. London, 1821; with some additions relating to the Motions of Waves, and of Sound, and to the cohesion of Fluids. (This volume, and the article "Tides," in the supplement to the Encyclopædia Britannica, Dr. Young considered as together containing the most fortunate of the results of his mathematical labours.)

An Account of some recent Discoveries in Hieroglyphical Literature and Egyptian Antiquities, including the author's original Alphabet, as extended by M. Champollion, 8vo. London, 1823; with a translation of some Greek Manuscripts on Papyrus, the most remarkable of which was Mr. Grey's "Antigraph" of an Egyptian original then lying on his table; the discovery of which singular coincidence was the immediate cause of the publication of the volume.

Hieroglyphics, collected by the Egyptian Society, folio. London, 1823—a collection of Plates of Egyptian Antiquities subservient to the study of Hieroglyphical Literature, lithographized at the expense of about fifty subscribers, but not at that time publicly sold. The second number, plates 16 to 40, contains nearly all that was known of the interpretation of the Hieroglyphics, the evidence for each word being exhibited in a comparative Index.

(This work was entirely carried on by Dr. Young; but the subscriptions not being adequate to the expenses, it was afterwards made over to the Royal Society of Literature, he undertaking to continue the supervision as before.)

A finite and exact Expression for the Refraction of an Atmosphere nearly resembling that of the Earth. Phil. Trans. 1824, p. 159; a computation derived from an optical hypothesis not exactly agreeing with the probable height of the physical atmosphere, but affording correct results.

Remarks on Spohn and Seyffarth. Quarterly Journal, Oct. 1826, in a Letter addressed to the Baron William von Humboldt.

A Formula for expressing the Decrement of Human Life; in a Letter addressed to Sir Edward Hyde East, Bart. Phil. Trans. 1826, intended to render the interpolation from the best observations more regular: it is followed by a correction of Dr. Price's mistake, respecting the periodical payments of annuities.

Practical Application of the Doctrine of Chances, as it regards the subdivision of Risks. Quarterly Journal, Oct. 1826; showing the limitations under which speculations on probabilities may be conducted with prudence.

Remarks on Mr. Peyron's Account of the Egyptian Papyrus. Quarterly Journal, Jan. 1827—the great Greek Papyrus of Turin; in which Mr. Grey's three contracts are cited and explained,—not two of them only, as had been supposed by Mr. Peyron.

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MISCELLANEOUS INTELLIGENCE.

§ 1. Mechanical Science.

1. *Eccentricity of Saturn's Ring.*—"The most interesting circumstance in astronomy just now is the eccentricity of the Ring of Saturn. M. Schwalz, of Dessau, first perceived it, and having written of it to M. Harding, the latter believed that he also could observe it. M. Harding informed me of the circumstance, and I also saw that which these two gentlemen had seen, as did likewise my assistants. I nevertheless persisted in believing that it was an optical illusion, occasioned by the shadow of the planet on the ring, and therefore wrote to M. Struve to decide the point by means of the superb micrometers attached to his great telescope." He had the goodness to measure, on five different days, the distance of the ring from the body of the planet, and he found, that what had been observed was not merely an appearance, but that Saturn was really eccentric with respect to his ring. You will see the details of these observations in my *Astronomische Nachrichten*," &c.—Schumacher, *Bib. Univ. Juin*, 1828.

2. *Resistance in Space to the Motion of Heavenly Bodies.*—In an account of the last appearance of Encke's comet in 1828, M. Gautier states, that the results then obtained accorded with those which Encke had previously procured, and which induced him, in 1823, to suppose the existence of a medium or ethereal fluid in space, of which the resistance, acting as a tangential force against the motion of the comet, would augment the power of the sun, and shorten the period of revolution. The most celebrated geometers, and even Newton himself, had already calculated the influence which such a resisting medium could exercise on the motions of comets and planets. They had found that its effect would be to diminish continually the eccentricity of their orbits, and to shorten the longer axes and the periods of their revolutions; that the length of the perihelium would suffer only a periodical change; and that the nodes and the inclination of the orbit would not be altered. In the case of Encke's comet, the two first effects have been decidedly produced, and there are two circumstances to facilitate the calculation; the first is, that this comet is always seen in the same point of its orbit, and near to its perihelium; and the second, that its orbit is subjected only to very slow alterations. Both these circumstances permit the supposition that the times of revolution (at least for some periods) diminish by an equal quantity, so that their diminution may be considered as proportional to the square of the times; the periodical variation of the perihelium may also be neglected without inconvenience. M. Encke supposes, with Newton,

that the ether, or resisting medium, is diffused through all space; that its density diminishes in the inverse ratio of the square of the sun's distance, and that the resisting force is always proportional to the square of the actual linear velocity of the comet.—*Bib. Univ.* xli. 3.

3. *New Method of measuring the Power of Telescopes*, by M. Valz.—The method consists in measuring the angle which the rays, from a body of known diameter, form when they leave the telescope. The sun, by reason of its brightness, appears the object best fitted for this purpose. Its image, therefore, formed at a given distance from the eye-glass, is to be measured, and divided by this distance, and by the double tangent of the semi-diameter of the sun. By making the distance equal to the cotangent of the sun's diameter, the number in parts of the scale employed, included in the extent of the image, will express the quantity of enlargement. Thus, in January, the image should be received and measured at 105 parts from the eye-end of the telescope; in July, at 109 parts; in April and October, at 107; but rigorously this quantity must be diminished by the diameter of the small image, which is formed at the point from whence the distance is measured. When the field of the telescope is smaller than the sun's diameter, we may previously trace a circle upon a screen which is then to be made to coincide with a part only of the edge of the sun. But it may be found preferable in such cases to employ the known diameter of the field of view in place of that of the sun; and if it has not been previously ascertained, it may be determined at the same operation by the time which a solar spot takes to traverse centrally the image received on the screen.—*Bib. Univ.* xli. 25.

4. *Brown's Active Molecules.*—Mr. Holland, the inventor of a microscope sold by Carey, of the Strand, has inclosed some of the particles described by Mr. Brown, as active molecules, between glass and talc, closing the whole hermetically so as to exclude, as much as care could do, all interference of external causes. Notwithstanding this, the motion continued equally vivid, even after ten days. The lens used had a focus of the thirtieth of an inch; and the particles were obtained, as we understand, from gamboge.

5. *Force of Running Water.*—A few facts and observations on the power of running water have been communicated to the Geological Society by Mr. Culley, one of its fellows. The heavy rains which fell during three days of August, 1827, swelled to an unusual height the small rivulet called the College, which flows at a moderate declivity from the eastern water-shed of the Cheviot Hills, and caused that stream not only to transport enormous accumulations of several thousand tons weight of gravel and sand

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to the plain of the Till, but also to carry away a bridge then in progress of building, some of the arch-stones of which, weighing from one half to three-fourths of a ton each, were propelled two miles down the rivulet.

On the same occasion, the current tore away from the abutment of a mill-dam, a large block of greenstone porphyry, weighing nearly two tons, and transported it to the distance of a quarter of a mile. Instances are related to occur repeatedly, in which from one to three thousand tons are in like manner removed to great distances in one day; and the author asserts, that whenever 400 or 500 cart-loads of this gravel are taken away for the repair of roads, that one moderate flood replaces the amount of loss with the same quantity of rounded debris.

Parallel cases of the power of water are stated to occur in the Tweed, near Coldstream.—*Phil. Mag. N. S.* vi. 65.

6. *Geological Hammer.*—Requiring a geological hammer, uniting the properties of power and compactness, Mr. Clarke selected one of Knight's trimming hammers, marked No. 5, and removing the handle from it, supplied another, formed in the following manner:—Three narrow slips of *soling leather*, of convenient length, were glued firmly together, so that the grain of the exterior pieces was in a different direction to that of the interior piece. The whole was rounded, and reduced from the extremity upwards, until it fitted the aperture in the hammer head, and there firmly secured by two small wooden wedges, driven in between the slips of leather. The handle was then strongly bound round with twine, from one end to the other. It was found to possess an elasticity almost perfect, and to preserve its shape.—*Mag. Nat. Hist.* ii. 247.

7. *Cement for hard Stones, Porcelain, and Glass.*—This cement is a natural product, which, without being abundant, is in sufficient quantity for all ordinary uses. The large snails which are found in gardens and woods, and are sometimes used for food, have a vesicle at the extremities of their bodies filled with a whitish substance, having a greasy and gelatinous appearance. If it be applied between two surfaces, whatever be their hardness or compactness, and the surfaces be brought together throughout, so strong an adhesion is ultimately occasioned, that if violent blows or thrusts be given to the substances, they frequently break elsewhere than at the junction. A flint about the size of the fist having been broken into two pieces, and rejoined by this means, being thrown with violence on the pavement, broke into fragments by fresh fractures crossing the former junction, but not going along it. All that is necessary to give this cement its full power is to allow it time to dry.—*Bull. Univ.* E. xii. 107.

8. *On the Structure of Metals, by* M. Savart.—M. Savart has

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lately made a beautiful application of M. Chladni's mode of exhibiting the nodal lines of vibrating elastic laminæ, in the examination of the structure and elasticity of various solid bodies, such as crystals, wood, stone, &c. &c.; or rather, resorting to M. Chladni's mode of rendering these lines visible by small particles placed on the surface of the plates, he has, by means of these lines, instituted an investigation of various physical properties of solid bodies; and since he has made this application, it has been found that the phenomena in question afforded a method of examining the internal structure of bodies, in some cases far surpassing in sensibility the power of polarized light, and applicable, in innumerable instances, where the latter would be of no avail.

Lately, he has applied the same means of investigation to the metal bodies, which up to this time have been considered, amongst solid substances, as those approaching the nearest to homogeneity. They have been regarded as assemblages of infinitely small crystals united without order, and no one suspected that, in an ordinary mass of metal, there might be differences of cohesion and elasticity as great or greater than those observed in fibrous bodies, such as wood. Nevertheless, experiment shews that circular plates of metal of uniform thickness, whether cast in moulds, or cut out of a solid block, or taken from rolled plates, act under the influence of sonorous vibrations as if they belonged to fibrous bodies, or those regularly crystallized. When a solid substance, perfectly homogeneous and uniform in its structure, is formed into a circular plate of even thickness in every part, if this plate be sustained horizontally by small surfaces of support, applied above and below at the centre only, and sand be sprinkled on the plate, and a violin bow applied carefully at the edge so as to produce the lowest possible sound, the nodal lines will be indicated by the sand arranging itself into two true diametrical lines, crossing each other at right angles; and, by varying the place to which the bow is applied, these diameters may be made to occur in any part of the plate. But if the substance be not similar in structure in every direction, then this mode of division cannot be effected in every direction, and the nodal lines become hyperbolas, which are frequently accompanied by sounds varying from each other, sometimes almost insensibly, at others differing by as much as a third, a fourth, or even a fifth. Hence is derived a means of distinguishing between substances which are of perfectly uniform structure, and those which vary in different directions, as crystallized and fibrous bodies.

Plates of gold, silver, copper, zinc; cast, forged, and rolled iron; tin, lead, bismuth, steel, antimony, and a great number of alloys, as brass, bell-metal, &c., have presented properties analogous to those of rock crystal or wood, when cut into plates oblique to the axis of elasticity or to the cleavage.

The fact being ascertained, that a plate of metal may always be considered as if in its structure it belonged to a crystallized system,

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it became a point to ascertain whether the masses of metal were regularly crystallized. By cutting plates in different directions, from different parts of the same mass of lead, and examining their nodal lines, they did not agree with each other, and it appeared that a mass of metal, considered as a whole, does not possess the structure or properties of a body regularly crystallized, though each plate cut from it acts as if it belonged to such a body. And even if a large plate, which resembles a regularly crystallized substance, be cut up into smaller plates, these, by trial, will almost constantly be found to differ from each other.

These and other facts shew clearly that the metals have not a homogeneous structure, and yet at the same time that they are not regularly crystallized, but that they have a sort of semi-regular structure, as if, at the moment of solidification, several internal distinct crystals were formed, considerable in volume, but not having their homologous planes parallel to each other, so that, though each crystal is of regular structure, the whole mass is confused.

By examining crystallizations of metal, and especially of lead, obtained in the usual way from quantities in the act of solidifying by pouring off the remaining fluid portion, this will easily be seen to be the case; and the distinct systems of crystals will be found to be the more extensive, the longer the time the metals have been retained in fusion, or the more frequently they have been fused.

A consequence of this structure is, that greater differences of elasticity will be observed in the same substance, by taking small plates than by using large ones, because then the number of crystals taken into the plates will be less. For the same reason a mass of metal examined in this manner will appear to be more regular in its structure as the mass is smaller.

Whether the plates of metal be taken from a large mass, or cast in moulds of the proper size, does not appear to make much difference; plates obtained in both ways will sometimes differ little, and sometimes much. The substance of the mould, its position, or the place of the jet by which the metal enters, appears to have no influence over the elasticity of the resulting plate, *i. e.* there is always one direction in which the resistance to flexure is the greatest. Neither does sudden cooling, nor the passage of an electric current along one of its diameters whilst in fusion and cooling, exert an appreciable effect on the phenomena; but the case is very different if a series of small blows be given to the mould as the metal solidifies: the formation of large crystalline systems is almost always then disturbed, and the resulting mass is of such uniform elasticity, that plates formed from it give only one sound, and the two nodal diameters can be produced across any part of it. It would be as interesting as important, to ascertain if the metals, whose crystallization has been thus disturbed, have a tenacity equal to what they acquire under ordinary circumstances, and whether they do not acquire some new properties which may render this process useful in the arts.

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Many causes, such as pressure, rolling, annealing, &c., may more or less alter the distribution of the elasticity of metals, but none of them appear to be of such a nature as to bring them to a homogeneous state. Thus, circular plates of lead, copper, tin, and brass, diminished to one-fourth of their first thickness by hammering, preserved nearly the same properties as they had immediately after fusion: their nodal systems were only a little changed in appearance, but the sounds accompanying them were at the same distance from each other as before.

Rolling produced similar effects, except that the crystalline systems were considerably extended in two directions parallel to each other, so that it does occasionally happen that plates of large size may present a structure approaching to regularity. This occurred with a plate of zinc, from which several discs were taken, so similar in properties, that the plate might be considered as regularly crystallized. A close examination of this plate in various directions gave such differences of elasticity as to lead to the conclusion, that the differences of resistance to flexure in different directions of the same mass of metal may be much greater than occurs even in some woods—as, for instance, the oak, beech, &c., and yet, as has been demonstrated in a former paper, the extreme elasticities in the beech are to each other as 1 is to 16.

The influence of annealing appears to be very feeble, or even nothing, upon metals which have not been compressed; for discs of copper, which had been exposed for hours to a temperature near the point of fusion, gave the same sounds as before. But when the metals have first been pressed, then annealing slightly alters their tone and the disposition of the nodal lines.

The phenomena observed in the metals were found to occur also with glass, sulphur, common resin, copal, amber, plaster, slate, &c.; but the interval between the two sounds belonging to circular plates of these substances is always very small, rarely surpassing a major semitone: the two modes of division also shewn by the nodal lines, although they affect a fixed position, are so near to each other as mostly to give a rectangular cross. It is to be presumed, indeed, that a heterogeneous structure will be discovered in almost every solid substance, except perhaps those which are deposits of pulverulent matter, as chalk, for instance, which appears to approach very closely to homogeneity. Amongst all the bodies examined by M. Savart one only was found, namely, sealing-wax, in which the right-angled cross of nodal lines could be produced indifferently in any direction; but this substance, being a simple mixture of gum lac, turpentine, and cinnabar, we may imagine how the latter pulverulent body may prevent the particles of resin from arranging themselves regularly.

M. Savart concludes his memoir by an observation, apparently applicable to all bodies which crystallize irregularly, namely, that immediately after they have become solid, they vibrate sonorously with much greater difficulty than they do a few hours or days, or

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even months later. It frequently happens, that a body which produced with difficulty only dull sounds, finally vibrated with such facility and energy, that its particles became disintegrated, and it flew to pieces upon the slightest agitation of its parts. From hence it appears to result that, during the solidification, many parts are, as it were, surprised in positions, from which they tend to depart, and that they acquire a permanent state of equilibrium only after a long time: thus, if a circular plate of sulphur be cast in a mould, and endeavours be made, immediately after cooling, to produce sound from it, no sound can be obtained. After some days, sounds, more or less dull, may be obtained; if then the number of vibrations for any set of nodal lines be determined, and then the plate be left for a month or two after that time, it will be found to sound freely, and further, the same set of nodal lines, or mode of division, will give a greater number of vibrations; the sound may thus be raised sometimes a full tone. It is well known that sulphur, which has been recently fused, does not immediately recover its former properties; but no one suspected that it required whole months, and even a longer period, to fully restore them.—*Annales de Chimie*, xli. p. 61.

9. *On the Solidification of Plaster, by* M. Gay Lussac.—Every one knows the property which plaster possesses, when deprived of its water by heat, of becoming solid with that fluid. The consistency which it acquires is very variable, and the purest plasters are precisely those which acquire least hardness. The cause has been attributed, in Paris plaster, to the presence of a few hundredths of carbonate of lime; but, without doubt, erroneously; for the heat necessary to bake the plaster is, in the small way, not above 300° F., and, in the large way, is never carried to the degree necessary to decompose the carbonate of lime. Besides, calcined plaster rarely contains free lime, and the addition of that base to those plasters, which have but little consistence, does not sensibly improve them. I think that we must search for the difference of consistency, which is acquired by different plasters, when mixed with water, in the hardness which they possess in their natural uncalcined state; a hardness which we cannot explain, but must take as a natural fact. That stated, I suppose that a hard plaster-stone, having lost its water, will acquire greater consistency when returning to its first state than a plaster-stone naturally softer. It is in some degree the primitive molecular arrangement which is reproduced. We find, in the same way, that when good fused steel has its carbon removed by cementation with oxide of iron, it will give, by a new cementation with carbon, a steel much more homogeneous and perfect than that obtained in the same circumstances by the cementation of iron.—*Annales de Chimie*, xl. p. 436.

10. *Formula for reducing a Mercurial Thermometer in high Temperatures.*—If q denote the degrees of a mercurial thermometer, n

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the number of degrees between the points of congelation and ebullition, s the number of degrees at the boiling point, and *cal.* q the degrees of the true augmentation of heat corresponding to the state q of the thermometer, the following expression is correct:—

$$Cal.\ q = q - \tfrac{1}{4}(q - s)\ 0.09 - 0.028\ \frac{q - s}{n}.$$

—*Annal. der Physik und Chemie.*

11. *Determination of the Mathematical Law, according to which the Elastic Force of Steam increases with the Temperature.* By M. Roche, Recueil Indust. p. 285.—It has already been ascertained—i. That a small increase of temperature augments considerably the elastic force of vapour. ii. That this force increased nearly in geometric progression, for each increase from 30° of Fahrenheit's scale, or 13⅓ of Reaumur's, or 16⅔ of the centigrade thermometer; the elastic force doubling successively for the successive augmentations of 16⅔ from the boiling point 100.

Nevertheless, it appears, from experiments made both in France and England, that the tensions of vapours depart from this law at high temperatures, and different empirical formulæ, more or less exact, have been proposed to represent the law of the elastic force; that of M. Laplace, inserted in the *Traité de Physique* of M. Biot, is of the form $F = 760^m \times 10^m + b\,i^2 + c\,i^3 +$ &c., in which F denotes the elastic force estimated in millimeters: 760^m the height of the column of mercury equivalent to the pressure of the atmosphere, and a, b, c, &c., constant coefficients, which M. Laplace endeavoured to determine by experiment; he found $a = 0.154547$, $b = 0.00625826$, &c.

Such a formula is very complicated, and to apply it to high temperatures, the terms i^3, i^4, &c., must be employed; i representing the excess of the temperature above 100°. But a simple formula may be obtained by observing that the elastic force of steam increases for each element of temperature by a quantity, which is in a ratio composed of the existing elastic force and of the increase which I denominate the *expansive heat*, and which is proportional to the product of the temperature by the density it would give to the vapour, or to the quotient of this temperature, by the volume which it tends to give to the vapour, according to Gay-Lussac's law of dilatation. We see, then, that the true law will be, that *the elastic force increases in a geometrical progression when the expansive heat increases in arithmetical progression*, and as this expansive heat, denoting by x the excess of temperature above 100, would be proportional to

$$\frac{100^\circ + x}{8 + 0.03\,(100 + x)} \quad \text{or} \quad \frac{100 + x}{11 + 0.03x}.$$

(0°, ⅜ being the coefficient of the dilatation or the increase of volume for each degree); and as

$$\frac{100 + x}{11 + 0.03\,x} = \frac{100}{11} + \frac{x}{11 + 0.03\,x},$$

the increments may be considered as proportional to the quotient, $\frac{x}{11 + 0.03\,x}$, and the elastic force may be expressed by the formula

$$F = 760^{m} \times 10^{\frac{n\,x}{11 + 0.03\,x}}$$

^{m} being a constant coefficient, and 760^{m} the pressure of the atmosphere; this formula, by employing logarithms, becomes

$$\text{Log } F = \log 760 + \frac{n\,x}{11 + 0.03\,x}.$$

If F be known by experiment, n will be obtained by resolving the preceding equation, which will give

$$n = \frac{11 + 0.03\,x}{x} (\log F - \log 760^{m}).$$

Now, if the values of n, which I call the logarithmic modulus of the elastic force of steam, be taken according to the table of elastic forces drawn up by the Institute, and inserted in the *Traité de Physique* of M. Pouillet, the mean value of n will be found $= 0.17$; the other values differing but little from it, the formula then becomes

$$\text{Log } F = \log 760^{m} + \frac{0.17\,x}{11 + 0.03\,x}.$$

In a memoir which I presented to the Institute, in the month of February, 1827, and which was referred to the examination of the committee charged with investigating high temperatures in steam-engines, I shewed how the modulus of steam of other liquids might be found, and their density calculated therefrom; and I found that the maximum of the elastic force of water takes place at a temperature of about 770°, where its density is nearly equal to that of the liquid, the pressure amounting to more than 4000 atmospheres.

12. *Destruction of Vermin in Ships by Steam.*—By letters from India, it appears that the application of steam has been found wonderfully efficacious in cleansing ships from vermin, and especially the white ant. A steam-boat (*the Comet*) was placed alongside a merchant-vessel, and steam from its boiler conveyed by a very simple system of pipes into the hold of the latter, the apertures to which were closed as well as they could be. The operation was continued for several hours; and there is no reason to believe that it was not effectual, and will prove a valuable process in the navy. Besides the direct object of cleansing the ship, another advantage accrued, from the discovery of every leaky place existing, by the oozing of the water through them, in which way leaks were made manifest, that could not be found out otherwise. The expense is

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said to be very moderate; and it is further stated to be the only process at present known, not even excepting sinking, which effectually destroys the white ant.

13. *Preservation of Butter.*—M. Thenard recommends the method used by the Tartars, which consists in fusing the butter in a water-bath at a temperature of 190° F., and retaining it quiescent in that state until the caseous matter has settled, and the butter become clear; it is then to be decanted, passed through a cloth, and cooled in a mixture of salt and ice, or, at least, in spring-water, without which it would crystallize, and not resist so well the action of air. Preserved in close vessels and cold places, it may be kept for six months as good as it was on the first day, especially if the upper part be excepted. If, when used, it be beaten up with one-sixth of cheese, it will have all the appearance of fresh butter. The flavour of rancid butter may, according to M. Thenard, be removed almost entirely by similar meltings and coolings.—*Bull. Univ.* D. xii. 155.

14. *On the Dilatation of Stone, by* M. Destigny (from the *Mémoire de la Soc. libre d'Emul. de Rouen.*)—The phenomena of dilatation by heat have been long known, and have been determined for a certain number of bodies. M. Destigny has extended this determination to the dilatation of stones observed by Bouguer in Peru, for bricks; by George Juan, a Spaniard, quoted by M. Rondelet (*Treatise on the Art of Building*); and lastly, by M. Vicat, at the hewn stone bridge of Souillac. The precise extent of this dilatation was unknown. To obtain it, an instrument of extreme delicacy was required, and M. Destigny himself constructed one, founded upon the difference of dilatation of stone and iron, or copper. He selected for his experiments stones about 365 millimeters in length, 165 broad, and 50 thick, and fixed at one end a metallic rod, by means of a foot fixed in a copper plug let into the stone, while the other end of the rod rested against the small arm of a very sensible lever, which moved a needle indicating the difference of dilatation. This small arm was 3½ mm. long, the other 100 mm. This last moved another lever, the length of the arms of which was 3 mm. for that against which it rested, and 100 mm. for the other, which was designed to point out on the arc of a circle, with a radius of 100 mm., the progress of the dilatation. The ratio of the space described between the small arm of the first lever, and the great arm of the second, was therefore as 1 : 1000. Transferring, then, the whole apparatus to a stove heated to 40, the dilatation must take place; and if it were the same for the stone and the metallic rod, the instrument would have indicated nothing; but the iron or the copper, having a much greater dilatation, put the small arm of the first lever in motion, and the amount of the difference of the dilatation might be read off on the arc of the circle. M. Destigny has thus found, for example, in one of his first experiments, the needle indicating 231.84 mm.

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The copper rod, of which the dilatation is exactly known, would have made the index describe 301 mm. if it had been fixed to a body insensible to caloric. He deduces that the 69.16 mm. of difference is due to the stone. These experiments, repeated alternately on different stone bodies with a rod of copper, and with one of iron, have given the following results:

	Absolute Dilatation.
Brass	0.00187821
Soft hammered iron	0.00122045
Carrara marble	0.00084867
Marble from St. Beat . . .	0.00041810
——— Solst	0.00056849
Pierre de Vernon	0.00043027
——— St. Leu	0.00064890

M. Destigny, wishing to know if the hygrometric state of the stone would produce any change in its dilatation, exposed to humidity a stone from St. Leu, and found that the dilatation was the same. One fact surprised him in this experiment,—it was the enormous quantity of water absorbed by the stone, almost amounting in volume to one-third of the stone itself.

The difference between the expansion of iron and stone accords with what was before known relative to the bad effects, and their causes, occasioned by introducing iron in certain situations into stone buildings. M. Destigny proposes to use compound and compensating bars of iron and copper for these purposes, but we doubt whether practice will sanction the suggestion.

15. *New Artificial Horizon.*—There are few more cumbersome instruments than the artificial horizon, in the manner in which it is usually constructed, so that an improved one, which we have seen with Mr. Newman, whose character for manufacturing the best philosophical apparatus is known to every friend of science in this country, promises to be of material advantage to the traveller. This instrument is comprised in a square box, the top of which is detached when the instrument is in use. The lower part contains the mercury, which is raised into the brass basin above by means of two screws, which previously kept together the parts of the box, acting upon a moveable bottom. The basin is rubbed over with nitrate of mercury, an invention of Professor Schumacher, of Altona, the effect of which is to make the quicksilver adhere to the sides of the basin, so that, with a little care, a perfectly level surface may be obtained, free from tremor, and well suited for the purpose for which it is required. By turning the screws, the quicksilver is then let down again into the reservoir; and the whole fulfils that indispensable condition in instruments designed for a traveller, that "the box shall not close unless all that it contains is locked, or the act of closing locks them."

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§ II. Chemical Science.

1. *Application of a high Temperature to the Evaporation of Liquids.*—A maker of chloride of lime consulted M. Longchamp on the means of vaporizing a large quantity of muriatic acid, and the latter recommended a tube of platina. In a trial, one kilogramme (2.21 lbs.) of acid was vaporized in a tube 3 lines in diameter, and heated for 8 inches of its length. A larger apparatus was then made, consisting of a tube 2 inches in diameter, and 42 in length; but as 4 inches were external at one end, 2 at the other, and 4 were included in the sides of the furnace, only 32 inches in length were heated, equal to a capacity of 96 cubical inches. With this, 1 kilogramme of acid was evaporated in a minute, or 1500 kilogrammes in 24 hours. A trial was made to support the platina tube by one of cast iron, but the latter fused, and caused the former, which had become very brittle at the fusing part, to break; being, however, cut and repaired by soldering with gold, it was as good and effectual as at first.

From the effects produced, M. Longchamp concludes, that at high temperatures the evaporation is not in proportion to the heated surfaces, but in proportion to the capacity heated; that in a vessel heated to redness, it is 8 times greater than at the usual temperature employed, and that tubes of platina may be used to vaporize large quantities of muriatic acid.—*Ann. des Sciences. Bull. Univ.* A. xi. 353.

2. *On the Specific Heat of Gases.*—In a memoir on the specific heat of gases published some time since, MM. de la Rive and Marcet gave their results and conclusions, the latter being, that all gases had the same specific heat. The results have been a little objected to, as having been obtained upon quantities of gas too small, and the authors have therefore repeated their experiments on a larger scale, and given the full detail in a memoir published in the *Bibliothèque Universelle*, N.S. xli. p. 37. We shall probably return to this paper; in the mean time we give the conclusions, which are precisely the same as in the former case, namely—

i. That at the same pressure, and in the same volume, all the gases have an equal specific heat.

ii. That in the same volume the same gas has a smaller specific heat, in proportion as it is subjected to a smaller pressure.

3. *Artificial Preparation of Ice.*—After numerous trials made by M. B. Meijlink with different salts, for the purpose of converting water contained in a tin vessel into ice, during their solution, he ultimately gave the preference to a mixture of 4 ounces nitrate of ammonia, 4 ounces sub-carbonate of soda, and 4 ounces of water. This mixture, in three hours, produced 10 ounces of ice; whilst with the mixture of sulphate of soda and muriate acid, he obtained ice only after 7 hours.

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4. *Odoriferous Lamp.*—The peculiar property which Dobereiner discovered in spongy platina, of causing the union of hydrogen and oxygen with extreme facility, and at temperatures much below those ordinarily required for the purpose, led him to examine into the chemical changes analogous to combustion, which other bodies could undergo by the same or similar means, and he soon found that alcohol vapour in the open air, and under the influence of the prepared platina, became converted into acetic acid. The experiment then became nearly the same as that founded upon Sir H. Davy's discovery of the power of a heated platina wire, in continuing the combination of combustible bodies and supporters of combustion without flame; and it ultimately gave rise to the formation of a lamp, which, containing alcohol, and prepared at the place of the wick with a piece of spongy platina, or, as Dobereiner calls it, sub-oxide of platina, or some other form of that metal, gradually converted the whole of the alcohol into acetic acid.

The lamp in this form has been used for a night-lamp; it gives light enough to see the time by a watch held close to it, and if more light be required, a piece of amadou may be carefully inflamed at it, and then a light procured in the usual way. M. Batka has proposed to use Eau de Cologne in place of common spirit of wine, and finds, that then the fragrance diffused is very grateful, being, in fact, occasioned by the actual formation of aromatic vinegar during the whole time the lamp burns. We doubt, however, whether a constant odour of this kind is desirable, unless indeed there be some bad smell to cover, and think that much more pleasure is derived from the inhalation of a perfectly pure and sweet atmosphere with the short occasional presence of fragrant odours, than from an atmosphere constantly, or for any long time aromatized.

5. *Electricity of the Solar Rays.*—(Letter from Sig. Carlo Mattrucci of Forli, to Professor Gazzeri.) "I hasten, Sir, to communicate to you some experiments which appear to me to deserve the attention of philosophers. Having been for a long time persuaded of the existence of electricity in the solar rays, I wished to ascertain the fact by experiment. Having for this purpose exposed to the sun a delicate condensing electrometer of gold leaf, I soon perceived the leaves diverge and open themselves also on that side of the glass case which was directly exposed to the solar action, as if they had been attracted by it. Being induced from this first fact to suspect glass in this situation electrified, I was anxious to know if this were the case: wherefore, having left some plates of it in the sun, in a few moments I touched them in different places with the ball of the electrometer, when a very perceptible divergence ensued, which, however, was much more apparent when I touched the plates, although lightly, with a flat surface, since the effects of the friction and the pressure did not afford a doubtful result. I con-

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cluded, then, that the solar rays had the power of electrifying glass, and it only remained for me to ascertain if this effect were owing to the real existence of electricity in these rays, or rather to the increased temperature of the glass, which I could easily determine by heating a plate of glass, and trying it with the electrometer. This I did several times, but never discovered any signs of electricity. I observed, also, that the glass plate exposed to the rays of the sun never became electric if placed beneath another glass plate, or if the face of the sun was obscured by the intervention of a cloud. These few experiments, which I have been induced to perform, seem to me sufficient to prove electricity in the solar rays. The influence of such a fact on the meteorological phenomena of terrestrial magnetism, and on so many other phenomena of nature, will, I hope, induce yourself and other philosophers to pursue the subject further."—*Antologia*, No. 100.

Forli, April 25, 1829.

Professor Saverio Barlocci of Rome, in a Memoir on the Influence of Solar Light, in the Production of Electric and Magnetic Phenomena, inserted in vol. xli. of the Giornale Arcadico, relates the following experiment he had performed, to ascertain the electric power of the solar light. Having decomposed it with a prism, he made the red ray and the violet ray fall upon two discs of blackened copper, each of which was attached to a copper wire. Two nuts of the same metal, sliding upon a vertical glass rod, and to which the two wires were attached, permitted their being brought near together, or removed at pleasure. Having suspended a prepared frog by the body to the upper wire, the legs were placed upon the lower one. The apparatus being thus arranged, whenever (the discs being respectively covered with the red and violet rays) a contact was formed between the extreme parts of the two wires, evident signs of contraction were observed in the frog.—*Note by Prof. Gazzeri.*

Having experimented two summers since, upon the solar spectrum, in exactly the same way, except that a very delicate galvanometer was used instead of a frog, no electricity could be obtained by means of an English sun.—M. F.

6. *Atomic Weights of Iodine and Bromine.*—The variation in the numbers given by different chemists for iodine induced M. Berzelius to experiment on the subject. Iodide of potassium was dissolved in very little boiling water, and a few drops of nitrate of silver added; the precipitate formed was redissolved by agitation, &c., and then the whole diluted with much cold water: an insoluble compound of silver fell down, which, if the iodide of potassium contained any chlorine, would evidently contain all that chlorine as chloride of silver. The purified and filtered solution was then precipitated by nitrate of silver, and the iodide of silver obtained, separated.

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When chlorine was passed over this iodide in powder at common temperatures, it was absorbed, rendering the yellow iodide white; by applying a slight heat, the iodine was driven off, and the chloride of silver left; but as this substance absorbs chlorine when cold, in small quantities, air was passed through the tube whilst the compound was in fusion, until all smell of chlorine had ceased. Five grammes of iodide of silver, decomposed by chlorine, gave 3.062 of chloride of silver; in a second experiment 12.212 of iodide became 7.4755 of chloride. Adopting the latter result, the number, according to Berzelius's series, is for iodine 789.145, 8.7011 for the density of its vapour, and 4.4193 for the density of hydriodic acid gas.

Berzelius makes the number for bromine greater than that given by Balard and Liebeg. To free the bromine from chlorine, it was first washed with much water, then part was changed into salt by ammonia, and the other part by zinc; in both cases the solutions were first purified by precipitation, with a little dilute nitrate of silver, and afterwards with more for the preparation of the pure bromide; the first precipitate was much paler than the second, shewing the presence and separation of the chlorine. The latter, after being fused, was a transparent yellow mass, and was decomposed with more difficulty by chlorine than the iodide. In each experiment, the chlorine was passed for three-quarters of an hour after red fumes had ceased to appear.

It is not said what weight of bromide gave a certain weight of chloride of silver, but only that the mean of two experiments gave 489.15 (Berzelius's numbers) for the atomic weight of bromine. The density of the vapour of bromine is said consequently to be 5.3933, and that of hydrobromic acid 2.731.—*Annalen der Physik*, 1828. *Ann. de Chimie.*

7. *Chloride and Iodide of Nitrogen.*—M. Serullas has stated to the Academy of Sciences of Paris, that the detonating substances usually considered as binary compounds of chlorine and iodine with nitrogen, contain also hydrogen, or, in fact, that they are the chloride and iodide of azote. The experiments proving this assertion are to be given hereafter.—*Le Globe.*

8. *Action of Iron on Ammonia.*—M. Despretz announced some time since, that when heated metals were subjected to the action of ammoniacal gas, they underwent a considerable change in their weight, in consequence of combining with some part of the ammonia. He now states that the weight of iron is sometimes increased as much as 11.5 per cent. in such an experiment, in consequence of the combination of nitrogen with it. If the temperature applied be too high, the nitrogen is expelled, and the compound destroyed.—*Le Globe*, April 14.

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9. *Effect of Muriatic and Sulphuric Acid on Hydrocyanic Acid.*—It is well known that hydrocyanic acid may sometimes be preserved for years unaltered, and, at other times, changes and undergoes decomposition in eight or ten days after its preparation. Whilst searching for the causes which influenced the spontaneous change of the acid, M. Kuhlman examined into the action occasioned by mixing other acids with the hydrocyanic compound. A mixture of muriatic and prussic acids being made and set aside, in twelve hours the bottle containing it was lined inside with yellow cubical crystals; some were less coloured than others, and those formed after a longer interval of time were colourless. The fluid part remained limpid, but was diminished to one half. The experiment was repeated with recently prepared prussic acid, mixed with its bulk of muriatic acid. No yellow crystals were obtained, but a large quantity of colourless ones, similar to those last obtained with the former liquor. With the exception of the colouring matter of the first crystals (apparently depending upon the smaller quantity of muriatic acid in that experiment), they all appeared to consist of muriate of ammonia only. No gas was evolved by the mixture and action of the acids.

Equal parts of sulphuric and prussic acids were then mixed, though with some little difficulty. A slight elevation of temperature took place: after two days, no crystals were formed, no colour produced, no gas evolved. Heat being then applied, a little prussic acid was volatilized, but that soon ceased; much inflammable gas, probably carburetted hydrogen, was formed, and the colourless liquid, on cooling, became a crystalline mass of transparent needles, easily recognized to be sulphate of ammonia.—*Ann. de Chimie*, xl. 441.

10. *Phosphorus in Vacuo.*—The following experiment is mentioned by Berzelius as due to Van Bemmeleer:—Sprinkle a stick of phosphorus here and there with resin and sulphur, put it under the receiver of an air-pump and exhaust: the phosphorus will become more luminous at the parts powdered than on the other parts, and ultimately inflame.

11. *On the composition of Phosphuretted Hydrogen, by* M. Buff.—The analyses of the two compounds of hydrogen and phosphorus, performed by MM. Dumas and Rose, not agreeing, M. Gay Lussac requested M. Buff to repeat in his laboratory the experiments of M. Dumas.

1. *Proto-phosphuretted Hydrogen.*

The gas which was subjected to the following experiments was prepared with phosphoric acid. It did not inflame in air, and dissolved in sulphate of copper without leaving the least residue. Decomposed in curved bell-glasses by antimony and zinc, it constantly yielded, for 100 parts, 150 of hydrogen. Potassium presented

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sensibly the same result if an excess of phosphuretted hydrogen gas were employed. Heated in the same way with very dry chloride of mercury, for 100 parts of the gas there were obtained 300 parts of muriatic acid gas, entirely soluble in water. The protophosphuretted hydrogen gas heated with great excess of pure oxygen, easily inflames and burns with an extremely vivid and almost insupportable light; so that it may be concluded, that all the phosphorus is changed into phosphoric acid; finally the residue, after many experiments, when collected together, did not discolour the red manganesiate of potassa. 25·25 measures of phosphuretted hydrogen, gradually burnt with 190 measures of oxygen, left a remainder of 139·5 measures of gas. In another experiment, 27 measures of phosphuretted hydrogen were decomposed by 160·75 measures of oxygen; there remained 101·75 of oxygen. These experiments evidently shew that 100 volumes of protophosphuretted hydrogen gas, to be completely decomposed, require 200 volumes of oxygen. All this proves that non-inflammable phosphuretted hydrogen gas contains once and a half its volume of hyrodgen, and the half of its volume of phosphorus vapour, a result which agrees with that of M. Dumas.

ii. *Phosphuretted Hydrogen.*

The gas examined was obtained from the decomposition of phosphuret of lime by water. It is very easily dissolved in sulphate of copper; but 100 parts have constantly left from 13·5 to 14·5 of insoluble gas. By heating it in a graduated bell-glass, it deposits phosphorus without changing its volume; but it ceases to burn in air, although it retains the property of dissolving in sulphate of copper. It decomposes very easily even at the ordinary temperature of the air, and sometimes at the very moment of its formation, so that there is no certainty of having it pure. Heated in a curved bell-glass with metallic copper, it increases its volume one half, like the preceding gas. This gas, mixed with three times its volume of carbonic acid, burns perfectly well in oxygen, and in air, and without leaving the least trace of phosphorus. In oxygen in great excess, it burns with a vivid and brilliant light almost like phosphorus; but in air, the light is much less brilliant, and in the end it becomes pale and bluish. In this last case, it is probable that there is formed a mixture of phosphoric and of phosphatic acid; but with oxygen in very great excess, the result is pure phosphoric acid.

Given quantities of phosphuretted hydrogen gas have been several times decomposed in oxygen in the manner stated; but the results of these different experiments do not agree very well with each other.

100 volumes of pure phosphoretted hydrogen have combined with	. .	204 of oxygen.
100	. .	270
100	. .	226
100	. .	240
100	. .	230
100	. .	232

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These experiments, however, clearly shew that perphosphuretted hydrogen gas contains more phosphorus than the preceding. The second alone tends to prove that two proportions of hydrogen are combined with one proportion of phosphorus. Different other methods have been tried, to determine more exactly the composition of this body, but they all failed, on account of the extreme facility with which it decomposes.

The phosphuretted hydrogen gases dissolve in sulphuric acid, as stated by M. Dumas. The solution of perphosphuretted hydrogen gas, at first clear, soon becomes turbid, and deposits phosphorus.

If caustic ammonia be introduced into the solution of this gas, the same quantity of gas is disengaged that had been employed. Nevertheless, on leaving this solution to itself, more and more phosphorus is deposited; the ammonia disengages no more gas, and the smell of sulphuretted hydrogen is perceptible. Perphosphuretted hydrogen, which had been left for six weeks in a bell-glass over mercury, did not any longer inflame on contact with the air; it retained, however, its peculiar odour, and the sulphate of copper dissolved 47.2 parts per cent. of it. On burning 18.135 volumes of this gas with 81.5 volumes of oxygen, there was a remainder of 66.7. If it be admitted that the gas not soluble in the sulphate of copper is pure hydrogen, which is very probable, on account of its remaining so long over the mercury, it will be found, from this experiment, that 100 parts of this gas are burnt by 200 parts of oxygen.

By passing a current of phosphuretted hydrogen gas into a solution of sulphate of copper a black precipitate is formed, and the solution becomes more acid than it was. Disengaged from the copper by sulphuretted hydrogen, and mixed with chloride of barium in excess, and then filtered, the ammonia yields no precipitate, which proves that it contains no phosphoric acid.

The black precipitate was dissolved in nitromuriatic acid, and the solution, after having been precipitated by chloride of barium, was evaporated to dryness. The remainder, dissolved in water, was treated with sulphuretted hydrogen to separate the copper. Ammonia added to the solution produced an abundant precipitation of phosphate of barytes. Nevertheless, the black precipitate appearing easily to undergo alteration, it was not thought necessary to subject it to a rigorous analysis.—*Ann. de Chimie*, Juin.

12. *Combustibility of Carbon increased by Platina and Copper.*—The following experiment is due to Wœhler:—Rasped cork is to be heated in close vessels with ammonio-muriate of platina, or verdegris, when a charcoal will be obtained, which, though it will not inflame spontaneously, does so if slightly heated, and then continues to burn of itself. The charcoal obtained from cork without these additions does not inflame at so low a temperature, nor continue to burn in small masses if once inflamed and left to itself.

This effect is analogous to that discovered by Dobereiner as be-

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hanging to platina; but, as regards copper, a more curious one of the same nature is shewn very easily by a common green wax taper. These tapers are coloured with verdegris, and, when burnt, the copper of the verdegris is reduced for a time in the wick. If such a taper be lighted, and the flame then blown out, leaving the wick glowing, combustion of the wax will still proceed, slowly indeed, but for hours and days together, until the whole of the wax is burnt, or until the combustion has reached some part where it is extinguished by the contact of neighbouring bodies. This does not happen with white tapers, and hence they are safer for ordinary use.

18. *Carbazotic Acid and Carbazotate of Lead.*—To obtain pure carbazotic acid, the finest indigo is to be crushed and heated moderately, with eight or ten times its weight of nitric acid: it dissolves, effervescing, and producing much nitrous vapour. When the scum has fallen, it is to be boiled, and fresh acid added, until no more nitrous vapour is produced, which is ascertained by covering the vessel from time to time with a capsule, and observing if red fumes gather beneath it. Care being taken on this point, neither resin nor artificial tannin is produced. When cold, hard, yellow transparent crystals are formed, which, taken from the mother liquor, are to be washed. When boiled with water, they dissolve, and the few drops of oleaginous liquid on the surface are to be removed by bibulous paper. Filtration and cooling then yield a large quantity of brilliant yellow lamellar crystals. To obtain this acid perfectly pure, it is to be redissolved in boiling water, saturated with carbonate of potash and cooled; the crystallized carbazotate of potash is to be washed and crystallized several times. The first mother-water, mixed with cold water, gives a brown precipitate, which is to be washed, then put into boiling water, and neutralized by carbonate of potash to give a fresh portion of the carbazotate of potash. This salt, dissolved in boiling water, and treated with sulphuric, nitric, or muriatic acid, gives, upon cooling, very brilliant, clear yellow crystals, in plates mostly triangular, which are the pure acid.

Sometimes the indigo and nitric acid will give no crystals; the liquid is then to be evaporated, and the acid separated from the residue in the manner above described. Much acid may also be obtained from the liquid which floats over the precipitate, by evaporating it a certain degree. The residue is to be reboiled with nitric acid, and then neutralized, &c., by potash, as before. Four parts of the best indigo yield one part of carbazotic acid.

The carbazotate of lead is readily formed from the pure acid and carbonate of lead. It is a yellow precipitate, scarcely soluble in water, and when dry, detonating strongly by heat or percussion. It is proposed to be used in percussion guns, in place of fulminating mercury; but nothing is said in comparison of it with the improved

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percussion compounds prepared in the metropolis.—*Allg. Zeitung*, Dec. 1828.

M. Liebeg finds that carbazotate of copper crystallizes in long rhombic needles of an emerald green colour. They are readily soluble in water, and, losing water by exposure to air, they become yellow.

A concentrated solution of carbazotic acid is precipitated by dilute nitric acid; it possesses this property in common with urea. The acid may be separated from the precipitate by washing with water.—*Heeneman's Repertoire. Phil. Mag.*, N.S., vi. 145.

14. *Decomposition of Sulphates in Water by Organic Matters.*—M. Vogel has made some direct experiments on this subject, and has found that a very weak solution of sulphate of soda, and a saturated solution of sulphate of lime, mixed with sugar, gum arabic, glyeyrrhizine, or an infusion of woad, and preserved for a long time in jars, away from the light, have been decomposed: sulphuretted hydrogen, carbonic acid, and acetic acid were formed; the waters had a strong odour of sulphuretted hydrogen, and, being boiled, gave that substance mixed with carbonic acid.

These observations may explain the formation of a great number of hepatic mineral waters. M. Dumenil had already observed the presence of acetic acid in some mineral waters, and the observation has been confirmed by M. Vogel.—*Ann. de Chimie*, xl. 483.

15. *Instantaneous Light Apparatus.*—Amongst the different methods invented in latter times for obtaining a light instantly, ought certainly to be recorded that of Mr. Walker, chemist, Stockton-upon-Tees. He supplies the purchaser with prepared matches, which are put up in tin boxes, but are not liable to change in the atmosphere, and also with a piece of fine glass-paper folded in two. Even a strong blow will not inflame the matches, because of the softness of the wood underneath, nor does rubbing upon wood or any common substance produce any effect except that of spoiling the match; but when one is pinched between the folds of the glass-paper, and suddenly drawn out, it is instantly inflamed. Mr. Walker does not make them for extensive sale, but only to supply the small demand that can be made personally to him.

16. *On the Analysis of Borax, by* M. Gay Lussac.—At the same time that M. Gay Lussac acknowledges that the process for the analysis of borax, devised by M. Arfwedson, is very correct in its results, still, guided by the opinion that it is always desirable to supply simple and exact processes for the use of the manufacturer, he recommends another to notice, which, at the same time that it is accurate in its indications, requires no more time than an alkalimetrical process. It consists in ascertaining the quantity of sulphuric acid necessary to neutralize exactly the soda contained in a given

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quantity of borax. Litmus paper, being rendered red by boracic acid, cannot be used for ascertaining the point of neutrality; but the tincture of litmus may, because that acid gives it a wine colour only, whilst free sulphuric acid gives it the red colour of onion peel.

Fifteen grammes of borax were dissolved in fifty cubic centimeters of water, and rendered of a clear blue colour by a little tincture of litmus. It was then saturated by degrees with sulphuric acid of a known strength from a graduated alkalimetrical apparatus in the ordinary way. The blue solution soon acquired a wine colour, which it retained until the close of the saturation, after which it quickly acquired the red colour due to the excess of sulphuric acid. But to operate with certainty, some precautions are necessary.

Whilst the solution of borax is hot, the quantity of boracic acid in solution renders the changes of colour less sensible. It is better, therefore, after having added nine-tenths of the acid necessary, to allow the temperature to fall before the neutralization is completed. As the changes of colour are most easily seen by comparative experiments, a little water should be tinted by the litmus, and a drop or two of the test acid added to it, to give it the colour of onion peel; this liquor is the standard with which the solution of borax is to be compared. As the sulphate of soda and boracic acid in the solution somewhat interfere with the action of the free sulphuric acid on litmus, a mixture of these two substances and water was made in the proportions of the experiment; tincture of litmus added to it, and then the quantity of acid necessary to give it the standard red colour noted. It required three drops, or a half division on the alkalimeter tube, to produce the effect, and consequently that quantity had to be abstracted from the whole quantity used in neutralizing the borax.

In operating on fifteen grammes of borax, 77.2 divisions of the test acid were required; this, diminished by three drops or half a division, gave 76.7 divisions, equal in the instrument used to 8.835 grammes of concentrated sulphuric acid: according to the analysis by M. Arfwedson, it should be 3.855.

The process was equally exact in the hands of MM. Welter and Pelouze, who tried it after M. Gay Lussac. It is frequently employed by M. Gay Lussac with advantage in analytical operations.—*Annales de Chimie*, xl. 399.

17. *Sulphuret of Silicium.* Van Mons.—By exposing a mixture of equal parts, of white sand and sulphur, at first to a moderate temperature, and then gradually to one as high as a red heat, a very hard grey porous mass is obtained, insoluble in water and in acids. Acted upon by a solution of caustic potash, it was dissolved, leaving only a few grains of unaltered silica. The liquid, being decanted and left to itself, gradually deposited a fine black powder, which,

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according to M. Van Mons, is an oxidated sulphuret of silicium.—*Bull. Univ.* A. xl. 478.

18. *On the Production of Artificial Ultramarine.*—The possibility of making ultramarine was first observed, when the blue matter found in a soda furnace was shewn by Vauquelin to have the properties of that pigment; and since then the experiments of Gmelin and Guimet have proved highly satisfactory and successful; but as the processes published are still expensive, M. Kuhlman has been induced to publish an observation he has made, in hopes it may assist in simplifying them. Whilst repairing a reverberating furnace for the calcination of sulphate of soda, he remarked that the brick bridge, separating the salt from the fire, was covered in different places with a coat of ultramarine. It appeared that, previous to the formation of the ultramarine, a sulphuret of sodium was produced, for the blue places were surrounded by small, brilliant, reddish-brown crystals of this sulphuret.

Whether the sulphate of soda is decomposed by the action of heat only, or by the simultaneous action of the heat and the fuel, or by the influence of the silica and alumina of the clay in the bricks, are questions M. Kuhlman could not answer; but the important point of being able to make artificial ultramarine, by the action of clay and sulphate of soda, was distinctly ascertained. It is also remarked, that sulphate of soda, prepared without any excess of acid, may be converted into sulphuret, and become reddish-brown by the sole influence of heat; but that when excess of acid is present, this decomposition cannot take place, and no ultramarine can be formed.—*Annales de Chimie*, xl. 439.

19. *Adulteration of Chromate of Potash; its detection.*—The chromate of potash has the power of combining with other salts of potash up to a certain extent, without any very sensible change in its form and appearance; and hence it has been sent into the market for the dyers, falsified by considerable quantities of sulphate and muriate of potash, the presence of which it has been difficult to ascertain. M. Zuber has, in consequence, and in consideration of the power of vegetable acids upon it, devised the following process for this purpose:—Add a large excess of tartaric acid to the chromate of potash to be tried, the chromate will be decomposed, and acquire, in about ten minutes, a deep amethyst colour. It will now, if pure, form no precipitate with the nitrates of baryta or silver, by which means the presence of muriate or sulphate of potash may be readily ascertained.—*Bull. de Mulhause.*

20. *Sympathetic Ink.*—A weak solution of nitrate of mercury forms a good sympathetic ink on paper. The characters become black by heat.

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21. *On the Detection of small quantities of Mercury.*—Mr. Smithson, some time since, described a very sensible test of the presence of mercury in solution, consisting in the immersion of a gold ring or plate into the solution, a piece of tin foil having been previously rolled round it, and a few drops of muriatic acid being added to the liquid. This small voltaic pile caused a deposition of any mercury present upon the gold, which became white in consequence. If, on applying heat to the ring or plate, the gold resumed its yellow appearance, the proof was considered perfect. M. Orfila, in applying this test, found some preparations gave all these appearances, and yet contained no mercury; and led by this to make various trials, he found that the tin foil might be dissolved by the acid, and then precipitated on the gold, giving the white appearance, which disappeared on the action of heat. He found that the gold and tin, if put separately into a mercurial solution, would not indicate the mercury by a white appearance on the gold. He found also that gold, whitened by the deposition of tin and of mercury, might be distinguished by pure strong muriatic acid, which dissolved the tin but not the mercury, and therefore restored the yellow colour of the gold in the former, but not in the latter case; but he thinks, that, though with this precaution and additional trial, the presence of mercury may be almost certainly ascertained in solution, yet still that the proof is not complete except the gold plate is put into a little glass tube, and the mercury driven off by heat, and being condensed above, is recognized as mercury in the metallic state. With these precautions, he admits that the use of Mr. Smithson's small voltaic pile affords the most delicate test known for small portions of mercury in solution.—*Ann. de Chimie*, xli. 92.

22. *On the Coloration of Golden Articles of Jewellery, by* M. Castellani.—The two best mixtures, according to M. Castellani, for the purpose of giving a good gold colour to articles of jewellery, are as follows:—

Muriatic acid at 22°	10 parts
Oil of vitriol	4
Crystallized boracic acid	2
Water	150

Or,

Acid muriate of alumina (liquid)	18 parts
Crystallized sulphate of soda	4
Crystallized boracic acid	8
Water	150

Either of these mixtures, with twenty grains of neutral muriate of gold, constitutes the bath, which is to be used in the following manner:—A large glass matrass, carefully luted at the bottom, is placed over a circular furnace, so as to have heat readily applied to it; the solution is to be put into it, and when at the boiling point, the pieces of jewellery, previously cleaned and picked, are to be

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introduced, suspended upon golden wires. After a few minutes, a copper wire is to be immersed, and left until the gold has acquired a deep colour; it is then to be withdrawn, but the articles still left in until they have acquired the colour necessary. They are then to be put into warm water, acidulated by sulphuric or acetic acid, to remove particles of oxide of copper, washed in clean warm water, and dried near a fire.

Generally, a single operation is not enough; for, as a long immersion produces harm from the oxide of copper, it is better to shorten it, and repeat the operation. The colour produced by several immersions is always the best; that by one long immersion is red, and often requires the article to be entirely cleaned and recoloured.

The mixtures above have been used for golden articles containing one fourth of copper; other alloys would probably require other proportions. When the articles are large and thick, the immersion should be longer than for small, thin, or narrow ones. As the bath is good in proportion to the gold it contains, when, by successive colorations, that has been removed, a few drops of muriate of gold should be added, and sometimes portions of the other constituents and of water. The copper wire is oxidized in the process, and sometimes covered with a film of gold, in which case it should be changed or cleaned. If an intense yellow colour is required, the immersion should be frequently repeated, and the copper brought into contact. If a pale colour is required, the last immersion should be at the boiling point, and the copper wire ought not to touch.

Bronze articles, gilt by amalgamation, may be coloured in the same way; but M. Castellani has not as yet determined the best mixture for the bath.— *Bull. Univ.*, E. xii. 23.

23. *Berzelius's Analysis of Platina Ores—Native Platina of Nischne Tagilsk in the Oural.*—This ore is of a dull grey colour, and contains many magnetic grains, some of which are polar. These were analyzed apart from the rest, and gave a difference in composition nearly constant.

	Unmagnetic.	Magnetic.
Platina	78.94	73.58
Iridium	4.97	2.85
Rhodium	0.86	1.15
Palladium	0.28	0.30
Iron	11.04	12.98
Copper	0.70	5.20
Osmiuret of iridium	1.96	0.00
Insoluble	0.00	2.30*
	98.75	98.86

* Mixture of osmiuret of iridium in grains and scales with sand.

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Platina Ore from Gorobiagodat in the Ourals.—This ore is not magnetic, and is remarkable for containing no iridium. The proportions were—

Platina	86.50
Rhodium	1.15
Palladium	1.10
Copper	0.45
Iron	8.32
Osmiuret of iridium	1.40
	98.92

The loss in these three analyses is partly due to osmium, which distilled over during the solution of the ore in acid.

Platina Ore from Barbacoas in the Province of Antioquia, Columbia.—This ore consisted of grains often of considerable weight, mixed with smaller ones. The composition of the largest was as follows:—

Platina	84.30
Rhodium	3.46
Iridium	1.46
Palladium	1.06
Osmium	1.03
Copper	0.74
Iron	5.31
Silica	0.60
Lime	0.12
	98.08

Ann. de Chimie, xl. 337.

24. *On the Specific Gravity of Alloys and their Point of Fusion, by* M. A. T. Kupfer.—The author commences with detailing the methods he employs to weigh his alloys; in these there is nothing new, and we shall confine ourselves to his final results.

Alloys of Lead and Tin.—The metals were combined in the simple or multiple proportion of the weights of their atoms, because the author thought there must exist a simple relation between the dilatation of these alloys and their chemical composition. The weights of the atoms were taken from the table of M. Berzelius, and the following are the mean results he has obtained:—

Proportion in atoms.	Observed density.	Calculated density.	Differences.
Lead	11.3303		
Tin	7.2911		
1 tin, 1 lead	9.4263	9.4466	0.0103
1 tin, 2 lead	10.0782	10.0936	0.0154
1 tin, 3 lead	10.3868	10.4122	0.0254
1 tin, 4 lead	10.5551	10.6002	0.0431
2 tin, 1 lead	8.7454	8.7518	0.0064

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Proportion in atoms.	Observed density.	Calculated density.	Differences.
3 tin, 1 lead	8.3914	8.3983	0.0069
4 tin, 1 lead	8.1730	8.1826	0.0096
5 tin, 1 lead	8.0279	8.0372	0.0093
6 tin, 1 lead	7.9210	7.9326	0.0116.

This table shews that tin and lead dilate when mixed. This dilatation increases from the alloys of 2 or 3 atoms of tin to 1 of lead; and if there be a point where there is no dilatation, it must be between these alloys; in fact it is found toward the middle, as the author ascertained directly.

Amalgam of Tin.—The author has ascertained anew the density of mercury; he has found it 13.5886, taking for unity that of water at its maximum of density. At 26° of the centigrade scale, it is 13.5350, and that of tin 7.2868. All the following experiments have been referred to the same temperature of 26°.

Proportion in atoms.	Observed density.	Calculated density.	Ratio.
3 tin, 1 mercury	8.8218	8.7635	1.006632
2 tin, 1 mercury	9.3185	9.2658	1.005685
1 tin, 1 mercury	10 3447	10.2946	1.004865
1 tin, 2 mercury	11.3816	11.3480	1.002960

The following amalgams were made in the ratio of volumes, at the temperature of 17:—Density of tin, 7.2911; density of mercury, 13.5569.

Proportion in volumes.	Observed density.	Calculated density.	Ratio.
1 tin, 1 mercury	10 4729	10.4240	1.00469
1 tin, 2 mercury	11.4646	11.468	1.00000
1 tin, 3 mercury	12.0257	11.9905	1.00294

These observations prove that tin and mercury in general undergo a considerable contraction by amalgamating; but this contraction is nothing when one volume of tin is alloyed with two volumes of mercury, for the small differences between the results of observation and of calculation, which are seen in the above table, may be disregarded.

Amalgam of Lead.—The following experiments are referred to the temperature of 17°.

Proportion in volumes.	Observed density.	Calculated density.	Ratio.
1 lead, 4 mercury	13.1581	13.1116	1.00355
1 lead, 3 mercury	13.0397	13.003	1.00303
1 lead, 2 mercury	12.8648	12.8147	1.00392

It is here the amalgam composed of one volume of lead to three of mercury, which undergoes the least contraction.

Another curious result which it is easy to obtain from these observations is, that the dilatation of all these amalgams by heat is less than that assigned by calculation, supposing that each metal preserves its own dilatation; it is therefore proved that the approximation of the molecules increases the resistance which their mutual attraction opposes to the effect of heat.

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The points of fusion of the alloys of tin and lead, which have served for the foregoing experiments, are as follow:—

Lead	334°
Tin	230
5 atoms of tin, 1 atom of lead	194
4 atoms of tin, 1 atom of lead	189
3 atoms of tin, 1 atom of lead	186
2 atoms of tin, 1 atom of lead	196
1 atom of tin, 1 atom of lead	241
1 atom of tin, 3 atoms of lead	289
2 volumes of tin, 1 volume of lead	194

25. *On a peculiar Principle in Blood, distinctive of its Source.*—This principle has been remarked and described, and its utility in medical jurisprudence stated by M. Barruel. Whilst preparing the colouring matter of blood according to M. Vauquelin's process, the clot of ox blood was boiled with a large excess of sulphuric acid of moderate strength, on which occasion a strong odour of beef was observed. Some time after, having occasion to operate upon the blood of a man who had taken opium, the fluid was first coagulated by heat, and divided, after which it was boiled with weak sulphuric acid: immediately so strong an odour of the sweat of man was evolved as to infect the whole laboratory, and render it necessary for the persons to leave the place. This and the former fact combined, induced M. Barruel to extend experiments on these subjects, and the following are the results.

i. The blood of each species of animal contains a principle peculiar to each. ii. This principle, which is very volatile, has an odour resembling that of the sweat, or the cutaneous, or pulmonary exhalation of the animal from which the blood was taken. iii. In the blood this volatile principle is in a state of combination, its odour being then insensible. iv. When the combination is broken, this principle is volatilized, when it is easy to recognize the animal to which it belongs. v. In each species of animal, this principle is more decided, or has more intensity of odour in the male than in the female; and in men, the colour of the hair accompanies certain variations in this principle. vi. This principle is in a soluble state in the blood, and may be found, therefore, either in the unaltered blood, or after the fibrine has been removed, or even in the serosity of blood. vii. Of all the means of setting this principle at liberty concentrated sulphuric acid has succeeded best.

To obtain these results, it is only necessary to put a few drops of blood or the serosity of blood into a glass, to add concentrated sulphuric acid, to the amount of one third or half as much as of blood, and to stir the whole together with a tube—the odoriferous principle is immediately rendered evident. By these means, M. Barruel can readily distinguish the blood from the following sources.

i. That of a man disengages a strong odour of the perspiration

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of man, which it is impossible to confound with any other. ii. That of a woman, by a similar odour much weaker, and resembling the perspiration of women. iii. That of the ox, a strong odour of oxen or a cow-house, or of cow-dung. iv. That of the horse, by a strong odour of the perspiration of the horse or of horse-dung. v. That of a ewe, by a strong odour of wool, impregnated with the perspiration of that animal. vi. That of a wether, by an odour analogous to that of sheep, mixed with a strong odour of the goat. vii. That of the dog, the odour of the transpiration of a dog. viii. That of a pig, by the disagreeable odour of a piggery. ix. That of a rat, by the bad odour belonging to the rat.

The same result has been obtained with the blood of various kinds of birds, and even with the blood of a frog, which gave the strong odour of marshy reeds, &c., and with that of a carp, which gave a principle smelling like the mucus which covers the bodies of fresh water fish.

Upon trials made to ascertain whether spots of blood could be distinguished, and referred to their source, M. Barruel found, that to a certain extent, a pretty sure judgment could be given, even after fifteen days or more. The spotted linen is to be cut out, put into a watch-glass, and being moistened with a little water, is to be left for a short time at rest. When well moistened, a little concentrated sulphuric acid is to be added, and stirred about with a tube, then by respiring near it, the odour may be perceived. M. Barruel is not sure that the distinction could be ascertained after more than fifteen days, and therefore recommends legal officers to allow of no delay in any intended experiments, which have to bear upon cases of judicial investigation.—*Ann. d'Hygiène Publique.*

26. *Preparation of Hartshorn Jelly.*—The following process is due to M. Ferrez: four ounces of rasped hartshorn is to be steeped in eight ounces of water, acidulated with sixty grains of muriatic acid for ten minutes, and then washed carefully in two or three waters. It is then to be boiled with fresh water for half an hour, pressed through a cloth, and the liquid filtered whilst hot. This fluid is the jelly, which being qualified by sugar or other ingredients and boiled slightly, gives upon cooling, a perfectly clear and good jelly for the table.—*Jour. de Pharmacie*, 1828.

27. *Braconnot's Indelible Ink.*—An account of this ink is given in our last Number*, but, unfortunately, M. Braconnot has been too hasty, and now finds that the ink does not deserve to be called *indelible*. With great candour he has written to M. Gay Lussac, and says, "in addressing some observations on a brown colouring matter to you, I was too hasty in announcing, according to some trials made in a hurry, that it might be employed advantageously

* See page 414.

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as an indelible ink; for having latterly submitted it to new trials, I was not long in convincing myself that it did not deserve that character, since the letters formed with it disappeared by successive macerations in chlorine and potash."—*Ann. de Chimie*, xl. 439.

28. *Preparation of Morphia, without the use of Alcohol.*—Having observed that it was easy to separate morphia from narcotine, by the use of very weak muriatic acid added to perfect neutralization, MM. Henri and Plisson founded upon it the following process. Five hundred parts of opium are to be divided into small strips and infused thrice, each time in 500 parts of water, at 80° or 100° F., with 20 parts of muriatic acid. The residue is to be pressed, all the liquor put together, and a very slight excess of weak solution of ammonia or caustic soda added. This deposit is to be collected and carefully washed. The mother-liquors are to be acidulated, concentrated, and decomposed in the same manner. Potash, soda, and ammonia retain a large quantity of morphia in solution, when the liquors are diluted, but much of it is obtained by concentration.

The deposit occasioned by the caustic alkalis is yellowish, and composed principally of resin, caoutchouc, morphia, and narcotine, coloured by a brown matter. It is to be frequently washed with water, slightly acidulated, and assisted by a moderate heat until the liquor ceases to be saturated; a slight excess is to be allowed. The liquor is then to be filtered and evaporated; it contains a little resin and extractive matter, and much muriate of morphia, (the part which remains unacted upon, contains with the resin much narcotine;) it is to be concentrated considerably, and when brown crystals have been formed, they are to be slightly washed and then purified twice by animal charcoal, and re-crystallizations from water. The muriate of morphia thus purified is to be dissolved in a small quantity of water slightly acidulated, and decomposed by a slight excess of ammonia; after which it is to be put upon a filter, washed, and dried. Four hundred parts of opium gave from 26 to 27 parts of morphia, free from narcotine. It is yellowish, but solution in alcohol and crystallization gives very white crystals.—*Jour. de Pharmacie. Bull. Univ.* A. xi. 382.

29. *On Vegetable Jelly or Pectic Acid*, by M. Vauquelin.—This principle, which was named by M. Braconnot pectic acid, and which he obtained in abundance from the carrot, after all which could be removed by water had been taken away, has been examined more recently by M. Vauquelin, who has described processes by which it may readily be obtained pure, and has pointed out certain remarkable properties belonging to it.

One method of obtaining the substance pure was to form the pectate of potash by acting on the carrot, freed from all soluble matter, as above, by a solution of potash; this was decomposed by solution of muriate of baryta, and the insoluble pectate of baryta,

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being pressed between filtering paper, was found to dry with great facility. This was then decomposed by excess of sulphuric acid, and the mixed precipitate of sulphate of baryta and pectic acid boiled with caustic soda by filtration, the pectate of soda was separated; and being precipitated by excess of muriatic acid, the pectic acid was washed, dried, and obtained perfectly white and pure.

A process for the preparation of larger quantities is as follows; and not requiring copious washings with pure water, may be carried on out of the laboratory. A yellow variety of Flanders carrot was rasped, pressed, and washed with common water, until the latter passed off limpid. Every 100 parts of the pressed carrot, with 5 parts of bi-carbonate of potash, were boiled in water the usual time, to form a clear fluid, and then pressed; a strong solution of pectate of potash was thus obtained, which, being decomposed by excess of muriate of lime, gave an insoluble pectate of lime. This was washed and treated with water acidulated by muriatic acid; and finally, with pure water. The pectic acid thus obtained was very pure, and far whiter than that procured by the use of caustic alkali.

If a very economic process be required, carbonate of soda may be used in place of bi-carbonate of potash—adding only a small proportion for the first decoction (about 5 per cent. of the crystals); very white pectic acid will be obtained. To obtain the whole quantity present, new decoctions, with fresh quantities of the carbonate, must be made.

The following remarks relate to some curious properties of this substance. If an excess of caustic potash be added to gelatinous pectic acid in a platina crucible, and gradually heated and agitated, the mixture soon liquefies and becomes brown. So soon as, by gradual evaporation, the whole of the water is dissipated, the saline matter, by careful management of the heat, becomes rapidly white. By examination it is then found that the alkali is nearly neutralized; and that, when dissolved, nitric acid evolves a little carbonic acid, but no pectic acid; and, by further examination, the potash will be found neutralized almost entirely by *oxalic acid*, formed at the expense of the pectic acid first added.

Pectic acid being boiled for an hour with cream of lime, a substance very like oxalate of lime was produced; but the existence of oxalic acid was not decisively ascertained. No pectic acid, however, remained unaltered.

This remarkable conversion of pectic acid into oxalic acid, under the influence of weak alkaline action, may be explained in two ways:—1. The action of the alkali may be something like that of nitric acid, on the same body, occasioning a derangement of its elements, and producing oxalic acid—a compound, which appears to be one of the last which occurs in the organic scale: or, 2. Pectic acid may be composed of oxalic acid and gelatinous matter, which

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is decomposed by the alkali, and the oxalic acid set free. There are many points of resemblance between the pectic and oxalic acids, especially in the insolubility of their salts; but, of course, the latter opinion must remain in doubt until the gelatinous matter has been separated, and pectic acid formed by its union with oxalic acid.

If the acid properties of pectic acid are due to oxalic acid, then the latter must exist in the carrot, combined with some base, and the presence of the organic matter, with which it forms pectic acid, must favour its separation by means of an alkali. This opinion appears probable, if we consider that the bi-carbonates, which have but a feeble alkaline action, disengage the pectic acid more readily than a caustic alkali; and also remark, that lime is found in the expressed juice. It may be admitted, also, that an alkaline force, however feeble it may be, is sufficient to develope pectic acid in the expressed carrot, by means of water and heat.—*Ann. de Chimie*, xli. 46.

30. *Glaucic Acid.*—This substance, discovered by Dr. Runge, and considered by him to be a new vegetable acid, occurs in several species of the *Dipsacus* and *Scabiosa*, and is said to be obtained thus. The roots of the devil's bit, or *Scabiosa succisa*, are to be dried, pulverized, digested in alcohol, and then ether added to the solution; white flocculi precipitate, which are to be dissolved in water, precipitated by acetate of lead, and the precipitate decomposed in water by sulphuretted hydrogen; by evaporation of the liquid, acetic acid is driven off, and glaucic acid remains. When dry, it is a brittle yellow mass, which reddens litmus, neutralizes ammonia, the neutral solution being at first yellow, but becoming bluish green upon exposure to air, in consequence of oxidation of the acid.—*Brande's Archives*, xxvii. 312.

31. *On the Formation of Acids in Vegetables*, by M. Vauquelin.—I have thought that, in a great number of cases, the developement of acids in vegetables was principally occasioned by the presence of alkalies. We find, in fact, the acids almost always neutralized altogether, or in part, by various alkalies, as lime, potash, soda, magnesia, and sometimes by vegeto-alkalies; and I do not know that the latter have ever been found in a free state in the vegetable kingdom.

The alkali which plays the greatest part in this respect is certainly lime, for it is most generally diffused, is most abundant at the surface of the earth, and powerfully attracts acids. It does not, certainly, enter into the organic kingdom in the state of lime, but as carbonate, which, without exerting any deleterious action on vegetables, still retains sufficient alkaline force to determine the formation of acids, and particularly the oxalic, which it prefers to all others.

We may thus, as I have said elsewhere, explain the effect of

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calcareous manures on vegetables. Immediately after its introduction into the organs of plants, the carbonate of lime determines the developement of an acid which decomposes it, and sets its carbonic acid at liberty, which, by means of light, is turned to account in the vegetable kingdom. From hence, it may be concluded that calcareous manures fill two important functions; namely, the division of the soil, and the nutrition of the plants.—*Ann. de Chimie*, xli. 59.

32. *Plumbagine, a new Vegetable Substance.*—M. Dulong has obtained a particular vegeto-principle from the roots of the *Plumbago Europæa*, Lin. This substance crystallizes readily from alcohol, ether, or water, in the form of numerous yellow needles. On the tongue it first produces a sweet taste, followed by a sharp acrid effect, extending to the throat. Its aqueous solution becomes cherry red by alkalies, subacetate of lead, permuriate of iron, &c. M but acids restore the yellow colour, and the plumbagine remains unaltered. Besides this principle, the root contains a black fatty matter, and gallic acid. As this root has been used in cases of itch, toothache, &c., and proposed to be administered as an emetic, it is supposed that its active virtues will be found in the plumbagine, to ascertain which experiments are in progress.—*Journ. de Phar.*

33. *Proportions of Oil in different Oleaginous Plants.* (Allgem. Hand. Zeitung.)—According to some experiments of MM. Schuebler and Bentsch on the oils of Germany, the following species of plants yield per cent. of oil—

Filberts	60	Summer rape	30
Garden cress	56 to 58	Woad	30
Olive	50	Carnelina	28
Walnut	50	Hemp seed	25
Poppy	47 to 50	Fir	24
Almond	46	Linseed	23
Navew	39	Black mustard	18
White mustard	36	Heliotrope	15
Tobacco seed	32 to 36	Beech masts	12 to 16
Kernels of plums	33	Grape stones	10 to 11
Winter rape	33		

34. *Colouring Matter of Lichen Roccella.*—M. Robiquet having undertaken a diligent examination of the Lichen Roccella, from which a beautiful blue colour, used in dyeing, is obtained, has discovered and separated from the other matter the colouring principle of this vegetable. The new and singular product which he has obtained has a very sweet flavour, is easily soluble in water, colourless, crystallizes in beautiful flat quadrangular prisms; by means of a moderate heat it may be volatilized without decomposing, and does not acquire the colouring property till it has undergone successively the action of ammonia and of common air.—*Antolo.* 101.

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Bark of the Soap Tree, or Quillai Saponaria.—This bark, if pulverized and shaken in water, soon yields a solution, frothing as if it contained soap. It is obtained from a tree in Chili, known by the name of *Quillai Saponaria*, the trunk of which is straight, and of considerable height. The wood is hard, red, and never splits. The bark is rugged, divided, fibrous, of an ash grey colour externally, and white within. It contains a peculiar substance, pungent, soluble in water and alcohol, and frothing with water; this matter dries into thin transparent plates. It contains also a fatty matter, united to chlorophyle; sugar; a brown colouring matter dissolved by alkalis; a little gum; a free acid; malate of lime; starch; lignine; oxide of iron, and a small quantity of the muriates and phosphates of potash and lime.—*Jour. de Pharmacie*, Mai, 1829.

Chemical Analysis of Green Oranges, by Dr. R. Brandes.—M. Brandes has found, in 2000 parts of this fruit,

	Parts.
Aurantine, or orange bitter, with traces of gallic, citric, and malic acids	26
Aurantine, with malate of lime, traces of resin, and gummy sugar	35
Sugar	24
Particular neutral crystallizable matter	6
Chlorophyle	4
Chlorophyle with stearine	7
Red, fatty, crystallizable colouring substance (Erythrophyle)	5
Albumen	15
Gum, with vegeto-animal matter	310
Citrate, malate, sulphate and phosphate of lime, sulphate and muriate of potassa, with traces of a salt of magnesia	12
Phyteumacolline, with malic acid, malate, and citrate of potassa	420
Phosphate of lime	3
Citrate of lime	12
Malate of lime	6
Ulmine (humic acid) with humate of lime	30
Vegeto-animal matter soluble in water, insoluble in alcohol	34
Vegeto-animal matter soluble in water and in alcohol moist obtained by caustic potassa	300
Vegetable fibre, with different mineral salts	140
Liquid substances, including volatile oil	480
	1869

Green oranges contain a particular bitter substance, aurantine or orange bitter, which is, with volatile oil, the principal seat of the active properties of this fruit.

In green oranges there is also found a peculiar crystallizable neutral substance, which deserves further examination, but the

proportion of which appears to be very small. They also contain a fat colouring matter, analogous to that which is met with in some yellow lichens, and which perhaps, by the progress of vegetation, becomes the orange yellow colouring matter of the ripe fruit, and forms, conjointly with the resin and the chlorophyle, the exterior pellicle of the orange.

The green fruit contain only a small quantity of resinous matter, but is remarkable for the considerable proportion of vegeto-animal matter soluble in water and in alcohol, which forms the principal constituent part of the internal medullary substance, and, conjointly with albumen, forms the greater part of the imperfectly developed seeds.

The internal medullary substance is in great part transformed into vegeto-animal matter by the action of caustic alkalies.

The most advantageous form in which green oranges can be employed in medicine appears, from this analysis, to be spirituous tincture.

M. Lebreton, in the chemical analysis of oranges which he has instituted, found a substance which he calls hesperidine; the same as the neutral crystallizable substance of M. Brandes. The results of these two chemists agree in other points also, particularly in what concerns aurantine; and yet, says M. Brandes, the two analyses were made independently of each other.—*Bull. des Sciences Médicales.*

37. *Chemical Analysis of the Mineral Waters of Gastrin, in Austria* (Circle of Salzbourg), by Professor Hünefeld, of Greisswalde. The waters of Gastrin contain,

Sulphate of soda .	0.1866	Silex . .	0.0433
Chloride of sodium	0.0369	Magnesia .	0.0013
Chloride of potassium	0.0183	Ox. of manganese	0.0018
Carbonate of soda .	0.0083	Oxide of iron	0.0063
Carbonate of lime .	0.0442	Sulph. of sodium	0.0038
Traces of fluoride of lime, and of phosphate of alumine . .			—
			0.3508
Water			999.6492
			1000.0000

Jahrbuch der Chemie und Physik.

38. *Analysis of Ipecacuanha Branca, Root of the Viola Ipecacuanha, by* M. Vauquelin.—The root of the Ipecacuanha branca is of a pale white, divided into many branches, of the thickness of a writing pen, much twisted, and contracted at unequal intervals. Its fracture is short, the odour of it disagreeable, the taste acrid and nauseous, the ligneous part is thicker than the bark. The substances which compose this root are as follows by weight:—Emetine, 1.50; Resin, 0.60; Gum, 0.20; Albumine, 0.30; Starch, 3.20; Matter crystallized in scales, 0.85; Ligneous matter, 7.00;

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fatty matter and wax, an indeterminate quantity;—Total, 15.95; Loss, 0.05.—*Journal de Pharmacie*, Juin.

39. *Chemical Examination and Analysis of the Flowers and Leaves of Yarrow*, Achillea Millefolium, L., *by* L. F. Bley.—The flowers contain, according to the analysis of M. Bley, in 2000 grains,

Volatile oil	1.9375	Gummy extract	2.0
Acetic acid	0.28	Malic acid	5.0
Vegetable albumen	64.0	Hard resin	21.0
Nitrate and muriate of potassa	42.0	Vegetable gum of the malates	315.0
Hard resin, with phosphoric acid.	13.0	Muriate of lime	2.5
Tanniniferous ext. with muriate of potassa	415.0	Chlorophyle	87.5

(All the above are extracted by water and by alcohol.)

Concrete albumen	52.0	Gluten soluble with difficulty	26.0
Artificial gum?	208.0	Tannin	5.0
Gluten	169.0	Vegetable fibre	320.0

(The above are obtained by the action of muriatic acid, and caustic potass.)

Products from incineration:

Sulphate and muriate of lime, with magnesia Alumine	8.0
Silica Oxide of iron, with oxide of manganese	8.0
Sulphur	a trace
Water	200.0
Loss	50.7825
Total	2000.00 gr.

In 2000 grains of the dried leaves there were,

Volatile oil	0.96
Acetic acid	0.48
Traces of sulphur	0.00
Vegetable albumen, with a trace of starch	24.00
Nitrate and muriate of potassa	44.00
Hard resin	12.00
Extract, with muriate, nitrate, and phosphate of potassa	352.0
Tanniniferous extract, with sulphate of lime	55.0
Gum	71.0
Chlorophyle	137.56

All these substances have been obtained by water, alcohol, and ether.

Concrete albumen	40.0	Vegetable fibre	360.0
Artificial gum?	371.0	Water	180.0
Gluten	265.0	Loss	37.6
Phyteumacolle	50.0	Total	2000.00

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The balsamic odour of yarrow is principally due to the volatile oil, and in part to the resinous substance. The taste depends upon the tannin, the extract, the resinous parts, the nitrate, and muriate of potassa. The sulphur is doubtless contained in the albumen. The bitterness of the leaf of yarrow, which Bucholz and Bernhardi attributed to a peculiar extract, is due rather to the chlorophyle, to the resin, and to the salts.—*Trommsdorff, Neues Journal der Pharmacie.*

40. *Researches respecting Platina*, by M. Dobereiner.—*a.* When chloride of platina is dissolved in about 300 parts of water, and the solution rendered sour with muriatic acid, if zinc, in a metallic state, be plunged into it, a powder is gradually precipitated, which chemists have always, but doubtless erroneously, (according to M. Dobereiner,) considered as pure platina; for in a state of dryness this body becomes warm, and assumes a brighter colour, if it be exposed to the air and moistened with alcohol; lastly, it becomes incandescent, particularly after having been treated with nitric acid, when (in contact with the air) it is exposed to a current of hydrogen gas.

b. When chloride of platina has been treated several times successively with absolute alcohol, at a mild heat, there is obtained at last a brown mass, which easily burns at a higher temperature, but which, dissolved in a great quantity of alcohol, affords a liquid very fit for being laid upon a glass, so as to form platina mirrors. For this purpose, dip the glass in the alcoholic solution, so that this last may be uniformly diffused; then bring it to a red heat in the flame of a spirit lamp. The coat of platina thus produced affords the brilliancy of a mirror, and adheres so well, that it is impossible to detach it. But if a mirror of this sort be dipped in water mixed with muriatic acid, and at the same time a plate of zinc be placed in the liquid, the whole coat of platina adhering to the glass dissolves almost instantaneously.

c. The decomposition of the oxide of carbon, by the dry oxisulphuret of platina, has always been observed by M. Dobereiner, although others have doubted the fact. He has found, that from 12 to 15 grains of the oxisulphuret (obtained by exposing sulphuret of platina prepared in the humid way to the action of the air during several weeks) are sufficient to decarbonate a cubic inch of the carbonic oxide gas, and that there remains exactly, in this experiment, half a cubic inch of carbonic acid gas, which proves (says M. Dobereiner), that the oxide of carbon is composed of equal volumes of carbonic vapour, and of carbonic acid gas.—The protocarburated and deutocarburated hydrogen act differently; they are absorbed in great part by the oxisulphuret of platina, and transformed into acetic acid, without any influence of atmospheric air.

d. The suboxide of platina affords an excellent method of discovering the presence of alcohol, dissolved either in atmospheric air or in any liquid; even the quantity may be determined by means of it. Thus, putting a drop of alcohol into a vessel of the size of

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8 or 4 cubic inches, and filled with dry air, and inserting into the vessel a few grains of the suboxide of platina, on a small plate suspended by a platina wire, at the end of a few minutes it is observed that the whole interior surface of the vessel becomes moist, and that this moisture collects into small drops, which make litmus paper red. This experiment succeeds well, particularly under the influence of light, and the suboxide becomes hot, although the acetic acid evaporates at the very moment of its formation. To determine the quantity of alcohol contained in a liquid, as in wine, beer, &c., a particular apparatus is to be employed, which communicates with a graduated glass jar filled with oxygen gas. In this apparatus, small quantities of the liquid containing alcohol are rendered acid in a few hours; and from the volume of oxygen consumed, the quantity of alcohol contained in the liquid subjected to experiment is calculated, (100 parts of absolute alcohol combine with 69.5 parts of oxygen to pass into the state of acetic acid.) M. Dobereiner promises ample details respecting his apparatus, to which he gives the name of *Acetogenator*.—*Jaheb. der Chemie und Physik*, No. 12.

41. *Crystallization of Sulphated Iron, by* A. H. Van der Boon Mesch, of Leyden.—These crystals, which are found in the mine of Grieshübel de Silberberg, in Bavaria, surpass in size, transparency, and regularity, all that it is possible to obtain by art, although M. Brongniart says, that they are rarely, if at all, found in nature; they are grouped round quartz flints of different sizes. Some crystals have the forms of oblique rhomboidal prisms, others of the *Fer sulfaté basé*, of Hauy. In general, the edges are not very angular, and the faces regular and united; their specific gravity is 2.037. They are of a clear green colour, soluble in cold water, and their solution is blackened by gallic acid. When they are exposed to the open air, the crystals alter; they become brown, and decompose into a powder of different colours, so that this mineral must be preserved with care. The first change it undergoes is to pass into the state of sulphate of iron, which perhaps has given rise to the opinion that this salt did not exist in nature. When this salt is heated in a retort, sulphurous acid is disengaged, which may be detected by Brazil wood paper; and when it is exposed to a white flame, it becomes black and magnetic. It is soluble in phosphate of ammonia, which transforms it into a glass sometimes red, sometimes yellow, which passes, after cooling, into a dull green. Melted again in this state with salt of phosphorus, it becomes of a very pure green. The same phenomena takes place with borax. The analysis which Berzelius, Bergmann, and Mitscherlich have made of this substance, gives 23.27 parts of oxide of iron, 28.89 parts of sulphuric acid, 38.45 parts of water. The formation of this mineral is attributed to the natural decomposition of sulphuret of iron in calcareous stone.—*Dyskrag. tot de Nat. Wet.* N. iv. p. 2.

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§ III. Natural History.

1. *Effects of the Sulphurets of Arsenic and other Metals on the Animal System.*—In order to test the opinion entertained by certain physicians, that sulphuret of arsenic is innocuous, M. Orfila made certain experiments with it which shewed clearly its deleterious nature. On applying 50 or 60 grains of the yellow sulphuret of arsenic to the thighs of dogs, these animals suffered in the same manner as by other arsenical preparations, and died in from 48 to 60 hours. The native orpiment of Tojova poisoned and caused death in two days. Death was produced in six days by 40 grains of native realgar, from Hapnike, in Transylvania. Hence it is shewn, that the sulphurets of arsenic, either natural or artificial, and when free from white arsenic, are still poisonous. An ounce of sulphuret of lead, introduced into the stomach of animals, caused no injury. Six gros (354 grains) of sulphuret of copper, given to a dog three months old, caused vomiting of a mucous and glairy matter, without affecting the health of the animal. The black and red sulphurets of mercury being well washed were also proved to be innocuous by similar experiments and results; and thus all that has been said of the deleterious action of these bodies is satisfactorily shewn to be erroneous.—*Archives de Medecine*, 1829.

2. *Decomposition of Corrosive Sublimate by Vegetable Bodies.*—According to the experiments of M. Fabian, the mucilage of quince-seed (*semence de coing*), and that of salop, decomposes corrosive sublimate the instant it is mixed with its solution; but the decoction of marshmallow does not produce the same effect, and the extract of liquorice only partially.

3. *Poisoning by Acetate of Morphia, and Recovery.*—A person in Paris took 24 grains of acetate of morphia dissolved in 1 ounce of water at six o'clock in the morning. In six or seven minutes the poison began to take effect, the man lost his senses, and remained without help until four o'clock in the afternoon. Then the master of the hotel broke into the room, and seeing the state of the person, immediately sent for MM. Orfila, Richard, and Tascheron, who thought the case so bad, they sent for the commissary of the *quatier*, expecting the man would die in a few hours. In the mean time assistance was given, bleeding in the arms was performed, water acidulated with vinegar was given, and also a very strong infusion of coffee. To the great surprise of all, these means were perfectly effectual, and the person ultimately recovered entirely.—*La Clinique. Bull. Univ.* cxvii. 247.

4. *Adulteration of Bread by Sulphate of Copper.*—The journals of Holland have, for some time, announced that sulphate of copper,

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or blue vitriol, was employed in that country to assist in the fermentation of bread; and at the same time that they pointed out the dangerous nature of this substance, they said that it was used because it was supposed to render the bread whiter, more compact, more healthy, and better fermented. The magistrates of Bruxelles caused thirteen bakers and five druggists to be brought before them for being concerned in this adulteration; and it appeared, i. that the employment of the process came from France, and that by the use of *blue alum*, a better fermented and whiter bread was obtained than by ordinary processes; ii. that the use of the process was announced in the public journals, and in a prospectus signed Frinch, which, being printed, was distributed, and stated that a patent secret for the preparation of leaven was to be sold, but without describing in what it consisted; iii. that the bakers inquired for the substance under the term of *blue alum*, and that the druggists gave, under this name, blue vitriol, or the sulphate of copper.

The magistrates decided that the druggists did not know the use to which the poisonous substance which they sold was to be put, although the frequency of demand, and the character of the buyers, ought to have raised their suspicion; they were therefore discharged, but they condemned the bakers (although they said they thought the substance innocuous) to five days' imprisonment, and a fine of fifteen florins.—*Jour. de Chimie Medicale.*

It is painful to observe the substance which forms our daily food thus adulterated and rendered poisonous, first by one article, and then by another, to satisfy the avarice of a set of tradesmen. No argument which they may advance, drawn from the competition in trade, or supposed innocence of the substance used, will avail them. They well know they are doing wrong, as is shewn by the secrecy in which they invest their proceedings; and besides their liability to be punished when their knavery is discovered, they must be branded with the character of a heartless set of people who do not hesitate for a moment to trifle dishonestly with the health of their fellow-creatures, when it serves their own pecuniary interest. It is also a sad thing that the practices of the bad should, by the just suspicions which they create in the minds of all persons, throw a shade over that portion of their fellow tradesmen who are willing to act honourably, which scarcely anything can remove.

5. *Rosacic Acid in Human Urine.*—M. Henry has observed in certain cases of acute rheumatism, accompanied by nervous fever, that the urine has been of a very red colour, and produced an abundant deposit on cooling. On analysing the secretion in such cases, he found that it was very acid, that phosphoric acid and phosphate of lime were very abundant, and that the uric acid had almost entirely disappeared and been replaced by rosacic acid in large quantities.—*Jour. de Phar.* xv. 226.

6. *Leech Bites.*—Dr. Lowendhart mentions a method of checking

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the profuse bleeding from leech bites, which is simple and effectual. The edges of the little wounds are drawn together with a fine needle and thread. The thread being drawn through the cuticle only, gives no pain, and the bleeding is at once suppressed.—*Journ. der Chir. Med. Jour.* vi. 86.

7. *On the Preparation of Food from Bones.*—This important subject has been long experimentally treated by M. D'Arcet, and has been brought before the public by the same philosopher, in a memoir published in the *Annales de l'Industrie* for 1829. It has been abstracted by the Editors of the *Annales de Chimie*, from whose account we take the following points.

Bones require to be divided, under the point of view in question, into two classes, those which are compact, flat, or cylindrical, containing but little fat, and which are sold at a high price to turners, button-makers, comb-makers, &c., and those which remain after this selection has been made, amongst which are included the spongy heads of great bones, and the extremities of the flat ones. The former class of bones having at all times their appointed uses in trade, it is the latter which is to be resorted to for food.

According to analysis, the latter bones contain per cent., earthy matter 60, gelatine 30, fat 10. The heads of large bones contain nearly 50 per cent. of fat. The bones of mutton and roast meat often yield a rancid or tallowy fat, and should be reserved for separate treatment.

100 kilogrammes (220.6 lbs.) of bones contain 30 of gelatine; 10 grammes of gelatine are sufficient to animalize half a litre (about a pint) of water, at least as much as is done in the best bouillon of the kitchen; therefore, 1 kilogramme of bones will prepare 30 bouillons of half a litre each, but 1 kilogramme of meat will only yield 4 such bouillons: from which it follows that, for equal weights, bones give to water seven and a half times as much animal matter as meat.

100 kilogrammes of butchers' meat contain about 20 of bones; the meat will make about 400 bouillons, but the bones would serve to prepare 600, so that in extracting all the gelatine from the bones of a given quantity of meat, 3 bouillons may be prepared with the bones, whilst usually both bones and meat only afford 2. With the bones contained in the meat consumed in the department of the Seine only, there may be prepared 600,000 bouillons daily.

The gelatine might be extracted by steam from the bones whilst whole, but it is better to crush them; but if this be done by successive blows, they acquire a bad smell: they should be moistened, and then crushed, if possible at once, either by channelled cylinders, or under a heavy weight. In the small way masses of wood will serve, being faced with channelled cast-iron plates. The pieces requiring a second operation should be remoistened. The bones should be used immediately after being broken, or else preserved in fresh water, or better in water nearly saturated with salt. If the

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bones be subjected for a time to a temperature of 260° or 280° F., or to high pressure steam, they break more readily; but then a part of the fat is altered, and the process should only be employed for those bones which contain no fat, or are old and dirty.

The preservation of the bones becomes an important matter, especially as they readily putrefy, and then one part of their jelly forms ammonia, which, combining with the undecomposed jelly, takes from it the property of gelatinizing when cold, and renders it soluble in water. They should first be freed from the fat, which otherwise in time renders them rancid. The broken bones boiled with water, yield much fat, but still retain enough to render them rancid. The process of separating the fat by saponifying it with soda, is not a good one, because the gelatine is also altered. Even the fresh bones may be preserved by being salted, but the products are injured, and the process expensive. The best method is that adopted by Plowden, for the preservation of meat, namely, dipping them into a strong solution of the juice of meat, or jelly, and drying them in the open air. For this purpose, a solution, containing one-fifth of dry jelly, was heated up to 180° or 190° F., and the bones, previously cleaned, broken, and freed or not from fat, dipped into it; they were then dried upon nets in the air, and again dipped, &c., once or twice more, to thicken the film of gelatine. They were then perfectly dried in a stove at temperatures from 70° to 80° F.

Papin proposed to separate the jelly from bones by the use of high pressure steam, but this process gives an empyreumatic flavour, and destroys gelatinizing power; long boiling of the rasped bones in water, at common pressures, is tedious and expensive, and does not give all the jelly. Fifteen years ago M. D'Arcet proposed to use acids in extracting the jelly, and thinks it was only because the process was not put into clever hands that it failed.

Whilst studying the process of Papin, M. D'Arcet removed the objections to it, by improvements in the apparatus required, for which he took out a patent. The most advantageous method appears to consist in exposing the bones to the action of steam, having a feeble pressure, the vapour condensing in the pores of the bone, first expels the fat, and then successively dissolves the gelatine.

The presence of the fat in bones complicates the process; under the influence of high pressure steam the carbonate of lime present acidifies it, and forms an insoluble soap, which opposes the solution of the gelatine. It is therefore necessary, first, to separate the fat, which is done either by boiling the broken bones in a covered cauldron with water in the usual way, or in exposing them to steam of ordinary pressure.

The extraction of the gelatine by uncompressed steam requires at least four days. The crushed bones freed from fat, or not, are to be placed in a cylindrical basket of tinned iron wire, which nearly fills a metallic cylinder, in which it is to be suspended. The latter

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being closed, steam of common pressure is to be sent in, which at first fuses the fat, and dissolves a little gelatine; after this, the pressure is to be increased. The liquids are to be withdrawn from the cylinder by means of a cock placed below. The gelatinous solution is obtained more or less concentrated, according to the slowness or rapidity with which the steam is condensed, and the divided state of the bones. The following are the points to be attended to. i. The bones should be broken in small pieces, and the smaller in proportion as they are more compact, more charged with fat, and require to be freed from it the more rapidly, or at a lower temperature. ii. The crushed bones should be previously freed from fat, either by means of boiling water in common boilers, or in cylinders by low pressure steam, or in water heated by steam. iii. The pressure of the steam should be diminished, and the duration of the operation increased, in proportion as it is required to obtain a purer and stronger jelly. iv. It is most economical to prepare very strong gelatinous solutions, which are afterwards brought to the proper strength by boiling water: to obtain this result, care must be taken to moderate the condensation in the apparatus. v. The degree of pressure most convenient is that of 960 millimeters (37¼ inches) of mercury, corresponding to a temperature of 223° to 224°.5 F. Stop-cocks placed on the vapour tubes serve to regulate the pressure at will.

The solution of jelly comes from the apparatus perfectly clear, unless agitated by violent expulsion. Being without taste, it may be sugared or flavoured at pleasure, for the preparation of table food. Reduced until it contains only two hundredths of dry jelly, it is equally charged with animal substance, as the best bouillon of the kitchen, and will serve either to animalize vegetable food, or, after being salted, coloured, and flavoured, to replace ordinary bouillon. The salt which is most advantageous in giving it an agreeable taste is, according to M. Braconnot, a mixture of 70 parts of common salt, and 30 chloride of potassium. If, after it comes from the cylinder, it is flavoured with vegetables, or the juice of meat and evaporated, gelatine or animal tablets are obtained.

The fat in bones is easily saponified by the action of high pressure steam; it is therefore better to separate it by boiling water, or even in water at a lower temperature; the fat is better the lower the temperature has been. The fat is easily given out in vapour, only slightly compressed, but the quantity saponified and remaining as an insoluble compound with the lime, amounts sometimes to four or five hundredths of the weight of the bones, which loss it is desirable to avoid.

An apparatus, consisting of four cylinders, has been put up at the Hôpital de la Charité. Each cylinder is a metre (39.37 inches) in height, one-third in diameter, contains about 40 kilogrammes of bones, and gives about 1000 bouillons per day.—*Ann. de Chimie*, xl, 429.

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8. *Process for Preserving Milk for any Length of Time.*—This process, invented by a Russian chemist named Kirceff, consists in evaporating new milk by a very gentle fire and very slowly, until it is reduced to a dry powder. This powder is to be kept in bottles carefully stopped. When it is to be employed, it is only necessary to dissolve the powder in a sufficient quantity of water. According to M. Kirooff, the milk does not lose, by this process, any of its peculiar flavour.—*Bull. des Scien. Agric.*

9. *Theory of Phrenology.*—From the minute examination and history of a case which has come before Dr. Albers, and in which the chain of evidence required seems to be very complete, and from a comparison of it with the doctrines of MM. Reil, Gall, Flourens, &c., Dr. Albers arrives at the following conclusions. "i. The development or imperfect development of the cerebellum is not always in direct ratio with the sexual instinct. ii. It appears that the lobes of the cerebrum may in part replace, in its function, the lobe of the cerebellum on the same side. iii. In order that the animal system may enjoy the full exercise of its movements, it is not necessary that the two lobes of the cerebellum should be equally developed, nor that the parts between them should be perfectly developed. iv. The disproportion in the development of the two lateral lobes of the cerebellum in this case, accorded with the state of the sexual organs and instinct," as indicated by malformation and other circumstances.

10. *Phosphorescence of the Sea.*—In a paper on the colouring principle of the water of the Baltic, M. Pfaff incidentally makes certain remarks on the phosphorescence which it exhibits, principally at the end of the summer until November. He confirms the opinion that the appearance is due to the presence of microscopic animals, and principally *infusores*. In support of this opinion, he quotes the careful observations of Dr. Michaelis, who has already determined several species, and noticed the most important circumstances affecting their phosphorescence. M. Pfaff observes, that if an electric current be passed through a tube filled with sea water recently taken up, there is immediately seen in it an infinity of brilliant points continually in motion, which remain visible only for a few moments. In general all experiments prove that these microscopic animals exhibit their light when acted upon by stimulants such as ammonia, acids, ether, and alcohol. It is also remarked that mechanical pressure produces the same effect upon the water, but that the phosphorescence is rarely perceived with water that is quiescent.—*Bib. Univ.* xli. 170.

11. *Wild Pigeons in North America.*—We have never before witnessed such multitudes of wild pigeons, as have appeared amongst us (Montrose) at the present season. Flocks extending

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miles in length have many days been seen passing over the hills and presenting a novel and interesting appearance. But what is most extraordinary, and causes us now to notice them, is their encampment about ten miles from this place, in a south-west direction, where they have built their nests and are raising their young. This encampment is upwards of nine miles in length, and four in breadth; the lines regular and straight; within which, there is scarcely a tree, large or small, that is not covered with nests. They cause such a constant roaring by the flapping of their wings, that persons on going into the encampment have great difficulty in hearing each other speak. Every thing throughout their camp appears to be conducted in the most perfect order. They take their turns regularly in sitting and in feeding their young, and when any of them are killed upon their nests by the sportsmen, others immediately supply their place. We are inclined to believe, that they have in part adopted Mr. Owen's community system, as the whole appears to be a common stock business. The *squabs* (as the young are called) are now sufficiently large to be considered by epicures better for a rich dish than the old ones, and they are caught and carried off by waggon loads.—*Susquehanna County Register*, May, 1829.—*Mag. Nat. Hist.* ii. 369.

12. *Changes in Animals in South America.*—All domestic mammiferous animals introduced into America have become more numerous than the indigenous animals. The hog multiplies very rapidly, and assumes much of the character of the wild boar. Cows did not at first thrive, but in St. Domingo, only twenty-seven years after its discovery, 4000 in a herd were not uncommon, and some herds of 8000 are mentioned. In 1587, this island exported 35,444 hides, and New Grenada 64,350. Cows never thrive nor multiply where salt is wanting either in the plants or in the water. They give less milk in America, and do not give milk at all if the calves be taken from them. Among horses, the colts all have the amble, as those in Europe have the trot: this is probably an hereditary effect. Bright chestnut is the prevailing colour among the wild horses. The lambs which are not from *merinos*, but the *tana*, *basta* and *burda* of the Spaniard, at first are covered with wool, and when this is timely shorn it grows again; if the proper time is allowed to elapse, the wool falls off, and is succeeded by short shining close hair, like that of the goat in the same climate. Every animal, it would appear, like man, requires time to accustom itself to climate.—*M. Raulin. Le Globe.*

Relative to certain changes observed in domestic animals by Dr. Raulin, in America, he concludes that, i. When animals are transported to a new climate, it is not the individuals only, but the race that has to be naturalized to the climate. ii. That in the course of this naturalization, certain durable changes are generally occasioned to the species, which place their organization in harmony with the

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climate under which they have to live. iii. That habits of independence rapidly convert domesticated species of animals into the wild species from which they have been taken.—*Mém. du Muséum, &c.* xvii. 201.

13. *Natural History of the Mole.*—Observations relating to the natural history of this animal have lately been made and published in the *Mém. du Muséum d'Histoire Naturelle*, xvii. 193, by M. Flourens, from which the following facts have been abstracted. Two moles were put into a vessel with earth at the bottom, and with roots of carrot and horseradish for food. The next morning only one mole was visible, the roots were not eaten, and on searching the earth, the skin alone of the other was found. It was opened throughout its length, beginning at the belly, and the bones and flesh were eaten.

The other mole was put into an empty vessel; it was excessively agitated and active, and appeared to be very hungry. A sparrow without its wing feathers was put to it, which at first pecked at the nose of the mole when the latter approached it, but after two or three times the mole darted on to the sparrow, thrust its nose into the entrails of the bird and detached the skin, at the same time devouring the flesh with a degree of fury. Putting a glass with water into the vessel, the mole drank abundantly once and again during its meal on the sparrow. The presence of the observer did not appear to interfere, in the least, with the motions of the animal.

The remains of the sparrow being removed, the mole was left to itself; an hour after it was lying quiet; in five or six hours it was very agitated, and appeared exceedingly weak, its belly pressed inwards, its sides depressed, its appearance breathless, its snout in continual motion—it appeared starved and ready to die. Another live sparrow was put towards the animal, who this time instantly jumped at it, and began to devour it as before at the entrails. After eating a little it drank, became of its usual size, and remained quiet. Next morning, all but the skin of the sparrow had disappeared, but still the mole seemed hungry and agitated; a frog was put in, the mole instantly fell upon and devoured it, beginning at the entrails as before. The mole was then left until it was very hungry, after which a toad was put to it: the mole instantly perceived it, but each time that it approached the toad, the latter swelled up, and the former turned away his snout as if disgusted. Roots of carrots, cabbage, lettuce, and nothing else, were then put in and left with the mole all night. Next morning the mole was dead, the roots scarcely touched, the bitten fragments still remaining. The mole, therefore, is not an herbivorous animal, and only destroys roots to get at the worms, insects, and larvæ within.

Three other moles were put separately, with vegetable food, as leaves, roots, &c. One died without at all touching the food, the other two also died, after slightly attacking the leaves, &c., and

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only in their search for animal food. On the contrary, moles were preserved for a long time, by giving them sparrows and frogs, or even butcher's meat, and sometimes with worms, snails, and wood-lice.

Two moles having been put into a room without food, some hours after, they were found the one pursuing the other, not a moment's cessation occurring; by the next morning, the stronger had eaten the weaker.

With regard to the time during which a mole can fast, from ten to twelve hours appears to be the maximum; at the end of that time they die. In three or four hours they become very hungry, and in five or six hours exceedingly weak. Eating always seems to refresh them perfectly, and, as happens with all carnivorous animals, they are very desirous of drinking when they eat: the contrary is observed with herbivorous animals. It is doubtful whether any other animal exists, which is obliged to eat at such short intervals as this.

From what precedes, it is evident that the mole is essentially a carnivorous animal. A new instance of the admirable relation which connects organization with manners, and functions with organs; and a new proof, that whenever there appears to be a contradiction between one of these things and the other, it is because the one or the other, the organization or the habits, have been badly observed.

M. Flourens remarks, that it will be interesting to observe in what degree the other *insectivores*, all classed in fact by M. Cuvier in the great family of *Carnassieres*, are really carnivorous; and especially what determinate modifications of their digestive organs correspond to the various modifications of their regimen: that of the hedgehog, for instance, which can eat fruits as well as insects, and that of the shrewmouse, which ought to live entirely on prey, if we judge by the shortness of the intestinal canal, which, as in true carnivorous animals, like the tiger, lion, &c., is only about three times the length of the body.

14. *On the Red Snow of the Arctic Regions.*—The following are parts of a letter by Mr. Nicholson, on this subject:—"In the summer of 1821, I had an opportunity of examining this substance, which has excited so much interest among naturalists in its native situation, and I am only surprised that those gentlemen, who first discovered it, should have any doubt as to the nature of its origin.

"On the 24th of July, whilst our ship was beset with ice, near Bushman's Island, I made a journey, accompanied by two other gentlemen, to Sowallick Point, in quest of the meteoric iron, which, we understood, was to be found in that quarter. We were disappointed in the object of our visit; but our mortification, on this account, was somewhat lessened by meeting, for the first time, with the crimson snow, which was described by Captain Ross.

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"Sowallick Point is formed by the projection of a small hill, from the high mountainous coast which bounds Prince Regent's Bay. The summit of this hill is covered with high masses of granite that have been precipitated at various periods from the cliffs above, whilst the side, which forms a gentle declivity towards the bay, was covered with crimson snow. It was evident, at first view, that this colour was imparted to the snow by a substance lying on its surface. This substance lay scattered here and there in small masses, bearing some resemblance to powdered cochineal, surrounded by a lighter shade, which was produced by the colouring matter, being partly dissolved and diffused by the deliquescent snow. During this examination, our hats and upper garments were observed to be daubed with a substance of a similar red colour; and a moment's reflection convinced us that this was the excrement of the little auk (*alca minor*); myriads of which were continually flying over our heads, having their nests among the loose masses of granite, which I have before described as covering the ridge of this little hill. A ready explanation of the origin of the red snow was now presented to us, and not a doubt remained in the mind of any that this was the correct one. The snow on the mountains of higher elevations than the nests of these birds was perfectly white, and a ravine, at a short distance, which was filled with snow from top to bottom, but which afforded no hiding place for these birds to form their nests, presented a uniformly white appearance. On the 2nd of August, I landed on Cape York, and procured a bottle full of red snow, and collected some of the dung of the little auks from the stones among which they had their nests; and my intention was to submit both to the examination of some eminent naturalists." A hasty summons to Antigua prevented this from being done.—*Mag. Nat. History*, ii. 321.

15. *On the Nemazoaires* of M. Gaillon.—A memoir, by M. Turpin, is contained in the fifteenth volume of the *Mém. du Mus. d'Hist. Nat.*, on certain marine productions, which have been considered, some as isolated animalculæ, the others as filamentous aggregations of animalculæ, similar to the former. In this memoir, M. Turpin contends against the system of M. Gaillon, and points out how he has been conducted to it. The conclusions of M. Turpin are as follows, and are drawn from observations made upon the *Navicula scalprum*, Gaillon, and the *Girodella comoides*, Gaillon.

i. Careful observations forbid the possibility that organized beings and free animals may re-unite and be engrafted one upon another, to constitute, in some degree, animals more complicated than themselves. ii. Such an idea, unless instantly dismissed from science, would tend, by its contagious nature, and the want of analogy which marks our times, to absorb all knowledge, and soon

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make us suppose that man himself has only an apparent individuality, composed by juxtaposition of microscopic animalcules. iii. All the species in nature are subject to the same laws of absorption, augmentation, and reproduction: all are produced by the extension of the substance of the mother who preceded them, and whose specific resemblance they are destined to perpetuate. iv. Both as an individual and as a species the *Girodella comoïdes* should be retained in the vegetable kingdom, because of its great analogy with the vegetable conferva, and the analogy of these with the cellular tissue of the superior orders of vegetables: for it is the product of a reproductive globule which extends in mucous filaments; and finally, there is gradually developed in these filaments vesicular globules, which ultimately become naviculares, and in which, at last, are produced other globules, destined each one to reproduce a new individual of the *Girodella comoïdes*.—*Bull. Univ.* B. xvii. 474.

16. *Gathering of Medicinal Roots.*—According to M. Kittel, roots should always be gathered in the autumn. This rule is without exception for all plants not annuals, with this difference, that the roots of biannuals should be gathered in the first year, whilst those of the rest may be gathered any year in their lifetime; but the roots gathered before the flowering year are always more charged with active principles than those which have often supported a stem and flowers, so that roots of the first, second, and third year are better than older roots. This is especially the case with aromatic and narcotic roots, as arnica, briony, gentian, belladonna, angelica, liquorice, sarsaparilla, dandelion, fennel, &c. &c. The volatile bitter, aromatic, nauseous, and, in general, all active peculiar principles, are more abundant in the cortical layers of the roots than in the woody part.

For these reasons, M. Kittel says, that fresh roots should never be allowed to be bought and sold for medicinal use, except in the autumn and winter.—*Repertorium der Pharmacie.*

17. *Drying of Plants.*—Always gather the root with herbaceous plants, and, if possible, select a specimen with part of the head in seed as well as in flower. Clear away the soil, &c., and, when quite dry, place the plant between papers, holding down the parts adjusted with the left hand, while regulating the remainder, and taking care to display any particular part which marks the generic or specific character. Instead of employing only two sheets of paper, lay three or four both above and below the plant, to absorb a greater portion of moisture, and place detached pieces of soft paper, folded in the required shape, size, and thickness on each side the stem, and more prominent parts, and, if necessary, over the petals and leaves. This will equalize the pressure, and

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add much to the beauty of the specimen. Different plants require different degrees of pressure, according as their texture is more or less firm, and a less degree of pressure should always be used the first two or three days of the process than when they approach desiccation, to prevent extravasation of the juices. Succulent plants may be left twelve or twenty-four hours (sometimes several days) before pressing, to render them flaccid and more conformable; and the interior parts of their stems, if bulky, and also all but the outer coats of tunicated bulbs, may be removed by making a longitudinal incision on the side that is to lie next to the paper. On the first removal of the moist paper, any irregularities in the disposition and folding of the leaves, &c., may be easily rectified, as they will then be partially flattened and more pliant; and, on every subsequent removal, the plants may remain exposed for half an hour or an hour on a table, and submitted to a gentle current of air. This will carry off much moisture and hasten the process; but they should be again placed under pressure when the petals begin to curl up or contract. The operation of changing the papers should be repeated morning and evening for the first few days, and afterwards once a day; as a general rule, the oftener the better, particularly plants with purple or blue flowers, from which confined moisture very soon discharges the colour. After the drying is completed, it will be well to place the specimens in an oven with a gentle heat, and under a very slight pressure, which, in the course of a night, will effectually drive off any remaining moisture that may have lurked in the central parts of thick stems, flower-buds, &c. If the oven be resorted to in the earlier stages of drying, the necessary pressure will confine the moist heated juices, and destroy the colours. I have found great advantage in dry weather in placing my press close to a window, opening the latter just enough to throw a current of air round the sides of the press to carry off the moisture as it is given out. My presses are of the simplest kind: flat boards or covers of books on which I place weights, bricks, or books to obtain the requisite pressure. Many layers of plants may be put under the same press, weighting it accordingly. These details may appear trifling, but when I recollect how slowly I surmounted one difficulty after another, I feel a pleasure in thinking they may be of service to some of your young readers, who, like myself, at the commencement of their botanical pursuits, may have no experienced friend at hand to direct them.—*J. E. Bowman. Mag. Nat. Hist.* ii. 245.

18. *Myrrh.*—The following communication, respecting myrrh, has been made to MM. Saigy and Raspail, editors of the *Annales des Sciences d'Observation*, by M. Bonastre.

I have succeeded in discovering that, in commerce, even at Paris, there are two very distinct species of myrrh: the true or ancient, and the false or modern; this last is said to be brought from

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India; it is that mentioned by Pliny, but the second of his species, which, according to this author, came by the Red Sea, and by caravans to Alexandria. I think that the second of my species, or the false, is that which MM. Ehrenberg and Hemprich discovered in Nubia, and which they refer to the *Balsamodendron myrrha*.

By the following characters they may be distinguished from each other. Equal parts of nitric acid will cause the developement of a colour rose, red, of the lees of wine, or sometimes violet, in the tincture of true myrrh; and a light yellowish colour in the new myrrh, which is the false myrrh. Of eighteen specimens which were presented to me for real myrrh, sixteen only evolved a red colour by nitric acid. The two others were not coloured; one belonged to bdellium, and the other was a sort of resin.

19. *On the different Genera and Species confounded with Cinchona.*—M. De Candolle has published a short notice on the different genera and species of bark, which have been confounded under the name of Cinchona; the following are his conclusions:—

i. It results from the enumeration made, that the 46 species of trees or shrubs, until now more or less confounded in books, compose eight distinct genera, namely,—Cinchona, Buena, Remigia, Exostemma, Pinkneya, Hymenodyctron, Luculia, Danaïs.

ii. What is known of the bark of these eight groups appears to indicate a decided connexion between the external forms and the medical virtues, and in particular, that although all these barks may be usefully administered in intermitting fevers as bitter or astringent, it appears that the barks of Cinchona only contain quinia, and that they probably are those which only are endowed with anti-intermitting properties.

iii. The yellow bark of medical men is produced from the *Cinchona pubescens*, and probably also in part from *C. Purpurea* and *C. Humboldtiana*. The orange bark from the *C. Lanoifolia*; the red bark from the *C. Scrobiculata* and the *C. Magnifolia*; and the pale bark of best quality from the *C. Condaminia*, whilst that of inferior quality comes from a mixture of many species.

iv. The eight genera obtained by the dismemberment of the old genus *Cinchona* are sensibly in the relation of the geographical distribution of these vegetables over the globe, *Luculia* and *Hymenodyctron* in the East Indies, *Danaïs* in the southern isles of Africa (Bourbon and France), *Pinkneya* in Carolina and Georgia, *Remigia* in Brazil, *Buena* and *Cinchona* in Peru and the Andes of Bogota. The genus *Exostemma* is an exception to this regularity; but it may be observed that true Exostemma lives in the Antilles, Pseudostemma in Brazil; and the Brachyanthes are divided between America and the Philippine islands, with this circumstance, that the species of the Philippines form perhaps a distinct genus.—*Bib. Univ.* xli. 144.

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20. *On the Duration of the Germinative Power of the Seeds of Plants, particularly of the Cucurbitaceæ.*—The Society for the encouragement of horticulture in Prussia proposes from time to time certain questions, to which it directs the attention of horticulturists. These small problems are an excellent method, on the one hand, of tracing a route for practical men, and of awakening in them an idea of easy researches of which they had not thought; on the other, of collecting a great number of observations, and of experiments on obscure or contested points of horticulture *.

The following is one of the questions proposed by the society:—"Is it true that the seeds of the melon and cucumber, being preserved for some years, yield a greater abundance of fruit?" Most observers remark that the plants obtained from the seeds of the preceding year produce many leaves, but few fruitful flowers, and almost entirely males ones; but that these same seeds, dried by the heat of the sun, or of a stove, yield more fruitful plants, and that it is particularly at the end of some years they acquire this property. These experiments vary from three to twenty years. The heat of the human body may be useful, but it must be used with discretion, or the germinative power of the seeds will be destroyed.

The author of this article has made experiments of the same kind on balsams and gillyflowers. He sowed at the same time some seeds of the last, some which were of the preceding year, others some years previous. The first came up much sooner than the second, and gave only simple flowers; the others produced only sixteen out of several hundred plants.

M. Schmidt employs seed from five to twelve years old; those of twenty years did not grow. Professor Sprengel of Halle says he obtained no fruit from seed a year old. M. d'Arenstorff, of Dreblesu, obtained fruit most remarkable for their flavour and size from seed of twenty years old. The observations of Professor Treviranus, of Berlin, have afforded the same result. A vigorous vegetation produces, in monœcious plants, male flowers in the greatest abundance, sometimes even exclusively. This has been proved, as far as regards the Cucurbitaceæ; but seeds which are too old produce an opposite result. He has seen seeds of five years old produce only female flowers; they were fecundated by male flowers of another bed, and yielded fruit.

M. Voss, head gardener at Sans Souci, sowed on the 7th of February, 1827, twenty-four seeds of a Spanish melon of the year 1790, being consequently thirty-seven years old, and he obtained eight plants which gave good fruit. This experiment, the most remarkable of all, will excuse our citing eleven others which he made

* Although the results obtained by these questions are designed solely for the advancement of horticulture, they should, in the first place, attract the attention of physiologists,—*Note by the* Editor.

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with seed of a less age, and of different species. Cucumber seeds of seventeen years old afforded the same results. M. Voss adds, that some seeds of the *alsea rosea* of twenty-three years old afforded very well conditioned plants.

"We admit, as incontestable, the above-mentioned observations. It is known that the seeds of different families retain, for a greater or less time, their germinative power; to cite only one example from among the leguminous plants—about twenty years since, we believe, fruit was obtained in the Royal Garden from a species of Phaseolus or Dolichos, taken from the herbarium of Tournefort."—*Verhandl. des Vereins zur Beförd. des Gartenbaues in den Preuss. Staat.*

21. *Influence of Chemical Solutions on Plants.*—This subject has been taken up experimentally by M. Wiegmann, whose object was to ascertain the influence of chemical solutions when applied to the *roots* of the plants, and taken in by absorption. His method was to put the liquids into vessels, into which were also then immersed the pots in which the plants were growing, the earth having been previously allowed to dry freely. In this manner it was found that the neutral solutions of acetate of mercury, acetate of lead, sulphate of copper, muriate of tin, muriate of manganese, nitrate of cobalt, nitrate of bismuth, tartrate of antimony, muriate of baryta, muriate of strontian, and solutions of white arsenic and dilute prussic acid, destroyed plants previously full of vigour, either in the course of a few days, or a week. On the contrary, solutions of the sulphates of iron and zinc, muriates of titanum, iron and lime, and sulphates of alumina and magnesia, produced no prejudicial action. When sought for, all the substances used were found in the plants, so that in opposition to what Mr. Murray had said, absorption had taken place by the roots.

Solutions of opium, hemlock, henbane, digitalis, and vomica nut, in the proportion of 20 grains of extract in 2 ounces of distilled water, poured into pots containing young plants of the family Chenopodus, caused death in from four to eight days.

Phillip's experiment of watering a plant with sulphate of copper, and killing it, was repeated, also the absorption of copper and its precipitation on a knife verified. Solution of 4 ounces of acetate of lead applied to a young willow did not kill it, probably because the carbonic acid disengaged by the roots precipitated the metal. A similar experiment, with 2 ounces of white arsenic, only made the tree to which it was applied grow more rapidly.—M. Wiegmann thinks, because the arsenic was in too small a quantity, and acted only as a stimulant.—*Bull. Univ.* B. xvii. 272.

22. *Professor Hanstein.—Terrestrial Magnetism.*—Letters have been received from Professor Hanstein and his companions, to the 19th of February. On the 12th of September they left Tobolsk,

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and travelled on sledges, the cold being at 40° R., so that the frozen quicksilver could be cut with a knife. On the 31st they arrived at Tornsk; on the 21st of January, 1829, at Krasnojarsk, and on the 7th of February, at Irkutzk, which is about 4000 versts from Tobolsk. They afterwards visited Kiachta, and crossed the frontier of China; but the most agreeable result is, that the desired object of the journey is accomplished, as the observations have proved perfectly satisfactory, and the magnetic pole is found. Centuries will perhaps elapse before Siberia will be again so thoroughly observed. When the letters were dispatched, it was resolved that the journey should be extended to Nertschinsk; from which place Professor Hanstein would return to Krasnojarsk. His companion, Lieutenant Due, was to go alone to Jakutzk, 2700 versts N. E. of Irkutzk, and perhaps proceed down the river Lena to the Frozen Ocean, and they intended to meet again at Jeniseisk in September or October.—*New Mon. Mag.* xxvii. 359.

28. *Destruction of the Caves of Kühloch and Rabenstein, in Franconia.*—Dr. Buckland has written a letter to the Editor of the *Philosophical Magazine*, inclosing another from Mr. Egerton, giving an account of the destruction of these caverns, as to geological reasoning. The cave of Kühloch was the most interesting and curious deposit of organic remains in Germany. It has been minutely described, and a drawing of it given in *Reliquiæ Diluvianæ*, and Professor Buckland is anxious to record its alteration and partial demolition, that the cause of its differing for the future from his description may never be forgotten.

Mr. Egerton says, "Lord Cole and myself are just returned to Schaffhausen, from a three weeks' visit to the antediluvian caverns of Franconia; and knowing the great interest you feel in their welfare, I write to inform you of the melancholy fact of the total destruction of the deposit of bones, in the caves of Kühloch and Rabenstein. His Majesty the king of Bavaria having announced his intention to visit Rabenstein, the owner of that castle has thought fit to prepare these two caves for his reception; in order to do which, he has broken up the whole of the floors, pounding the larger stones and bones to the bottom for a foundation, and spreading the earth and finer particles to form a smooth surface over them. Conceive our horror on arriving at Kühloch, at finding thirty men at work wheeling out the animal earth, to level the inclination of the entrance, by which you have so satisfactorily explained the phenomenon of the absence of pebbles and diluvial loam in this remarkable cavern. There was not a bone to be found there when we arrived; however, with a little management we contrived to obtain two beautiful fragments of lower jaws of hyæna, besides some very good bears' bones, and one tibia that had been broken during the animal's life, and the sharp edges of the fracture rounded off by the absorbents into a smooth stump.

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We likewise procured from one of the workmen, teeth of a fox, of a tiger, and molar tooth of the right lower jaw of a rhinoceros—all of which he said he picked up in Kühloch."

In the cave of Rabenstein, they found very few bones, but a great many old coins and iron instruments.—*Phil. Mag. New Series*, vi. 92.

24. *On the Progress of Storms in the Department of the Loiret.* By the Count de Tristan. (Abridged from the *Annales de la Société Royale des Sciences, &c. d'Orléans.*)—The data on which the Count de Tristan has depended are, i. Notes collected by him on the progress of storms, for the most part unaccompanied with hail, which he has been able to observe. ii. In a chronological recapitulation of all the places ravaged by hail in the department, during the last sixteen years, without any other information relative to the progress of the clouds, which occasioned these devastations. From these notes, it appears there have been fifty-one days in which serious mischief has been done by the hail in sixteen years, from January 1, 1811, to the first of January, 1827; but on several of these days there have been two and sometimes even three destructive storms, which have appeared at opposite points of the department. M. de Tristan, therefore, thinks he has determined sixty-four distinct storms; among these are twenty-six, the directions of which are considered known with sufficient exactness, others with much probability. A map shews the places which these storms have ravaged. There remain thirty-eight, which, having devastated only one or two isolated communes, have left no traces of their direction. The direct result of these observations is, that the mean direction of the storms in the department of the Loiret appears to be from the south-west, one quarter west: to this deduction, however, the Count has not confined himself; he has deduced other more general results, which, not being founded on a sufficient number of facts, we do not consider as altogether established, but which it is useful to know. The results are given in the form of aphorisms.

Aphorisms respecting the Progress and Intensity of Storms in Level Countries, or Countries intersected by Vallies of inconsiderable depth.

i. Storms are attracted by forests. ii. When a storm reaches a forest, (*a*) If it be very obliquely, it glides along it; (*b*) If it come almost directly against it, (*b* 1) It is either narrow, in which case it turns it, (*b* 2) or it is broad, in which case the storm may be totally stopped. iii. Whenever a forest, being rather in the way of a storm, has the effect of turning it aside, the velocity of the storm appears retarded for a moment, and its intensity is increased. iv. A storm which cannot deviate sufficiently, nor get round a forest, and which is in the case (*b* 2) of aphorism ii. (*a*) exhausts itself along it; (*b*) or if at length it pass over it, is much weakened.

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Sometimes, in this last case, it resumes its force a little further on.

v. The forests attracting, but not always arresting the progress of storms; it may happen, that storms, which were proceeding at a distance from each other, through the attraction of a forest are made to approach, and hence are disposed to unite either before or after passing over the forest. In this last case, a storm may appear stronger after having passed a forest, and when it goes off from it, than it was before arriving there—a natural effect which agrees with other observed phenomena, but which may deceive and appear in contradiction to aphorism iv. (*b*).

vi. A storm may follow a great river or a valley, provided, however, that it does not turn it considerably from the direction it would otherwise take.

(*a*) If this direction is nearly parallel to the river or to the valley, it will coincide with it exactly.

(*b*) But the approach of a forest, or rather a sharp turn in the river, or in the valley, will make it abandon it.

vii. A storm, the direction of which crosses that of a river or a valley, meets no obstacle therein and no delay.

viii. One stormy cloud attracts another not far distant, and makes it deviate from its route. There is reason to believe the action is reciprocal, and, consequently, that the deviation of each cloud is inversely as its power, allowance being made for accessary circumstances.

ix. One cloud attracted by a stronger one accelerates its motion as it approaches the principal storm.

x. When there is an affluent cloud, which was itself occasioning destruction—

(*a*) It ceases its ravages occasionally when approaching the principal storm—a consequence perhaps of the acceleration of its progress. . . . . .

(*b*) But after the junction, the mischief usually increases.

. . . . . .

25. *Peculiar Phenomena of Humidity.*—In the Memoirs of the *Petersburg Academy*, it is stated that, in the district of Gori, in Russia, at the foot of the Ossetin Mountains, there is a hill, on the stony surface of which, the humidity that exudes from the rock in summer and in fine weather is converted into ice, of a thickness proportionate to the heat of the sun. This ice disappears in the night, or during cloudy weather so completely that the rock is scarcely damp. The water obtained from the melted ice appears, upon analysis, to contain only a very small quantity of lime, and no other foreign matter.—*N. M. Mag.* xxvii. 311.

26. *Meteorology.*—M. Flaugergues, who has studied very closely the action of the moon upon our atmosphere, by observations carried on during twenty years, has found constant a certain relation between the number of rainy days and the phases of the moon. A

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constant observation, he says, has proved that it rains more frequently when the barometer is low than when it is high. On the other hand, observation shews that the barometer is lower in the first quarter of the moon than in the last, and lower when the moon is in perigee than when it is in apogee. From which it necessarily follows that there ought to be more rainy days in the first quadrature of the moon than in the second; and, similarly, there ought to be more rainy days when the moon is in perigee than when it is in apogee; which is in perfect concordance with numerous observations.—*Antologia*, No. 101.

27. *Decomposition of Rocks.*—The decomposition of rocks into globular masses has often been noticed. Dr. Klipstein quotes a case in Wetteravia, at Roosfield, near Holzheim, where schistose basalt is surmounted by prismatic basalt. The upper parts of the prisms are decomposed into small globes, which are more regular the nearer they approach to the surface. At Fauerbach, near Friedberg, a basaltic colonnade rises through diluvial sand; the columns are everywhere of the same thickness, and are divided into *balls*, which increase in regularity from below upwards. The basalt passes into wacke.—*Hertha. Bull. Univ.* B. xvii. 321.

28. *Swedish Iron.*—The quality of Swedish iron, and consequent demand of it for particular purposes is well known: at the same time, the quantity produced is but small. The following is the sale for 1828, given in tons:—

United States	9409	Russia	330
Germany	6676	Brazil	269
Great Britain	5753	Malta	142
France	5096	Spain	64
Portugal	3200	Antilles	59
Denmark	1771	Italy	40
Holland	1436	Norway	35
East Indies	893		
		Total	35,212 tons,

which, in the ports of Sweden, is worth from £400,000 to £410,000.—*Revue Ency.* xlii. 529.

29. *Commerce of the Sandwich Isles*, in 1828.—The *Berlin Gazette* contains the following extract from a letter written and dated March 4, 1828, from Honoluno, in Oahu, one of the Sandwich Islands:—

"The merchant vessels which go from America to the East Indies, find sale here, more or less advantageously, to the amount of 20,000 or 30,000 piastres of their cargoes, in glass, cloth, linen, &c. &c. Civilization and commerce have made great progress in these islands during some few years past. To this time the indigenous sandal wood has been the principal article of exchange, and

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has sufficed to purchase the importations; but this commercial source is ready to cease from the great activity with which it has been drawn upon; the natives, therefore, will be obliged to cultivate cotton, tobacco, sugar, and other similar products, which are appropriate to their soil and climate. In June, 1827, the importations rose to 220,000 piastres, and the exportations to 180,000. The money in circulation, in the island, is estimated at 200,000 piastres, which is not exported, because the sandal wood offers greater advantages. From 100 to 120 large commercial vessels have entered the port this year.—*All. Zeitung*, Nov., 1828. *Bull. Univ.* F. xviii. 348.

30. *North America. Kentucky. Fortifications attributed to the Indians.*—The line of calcareous rocks, which extends to the west, includes caverns of extraordinary depth. There is one named the Cave of the Mammoth, in Warren County, which has been explored by different persons for the length of ten miles, without their having found the end of it. It is the bed of a subterranean river, the water of which has ceased to flow. In the same district there are hundreds of cavities of a similar kind of large dimensions, and very curious: they are, in general, strongly impregnated with nitre. All this country, and particularly the plains of the Ohio, contain numerous defensive works, fortifications, terraces, which are almost always met with near the ruins of a village, or hillocks which have served for cemeteries. To judge by the appearance and age of the trees which grow in these places, their formation must be placed, at least, five or six hundred years back. It has, for a long time, been sought to discover, in these remains, the traces of another and more civilized people than the Indians of North America; but the facial angle of the skulls obtained from the neighbouring eminences, the shells, the amulets, the idols, the cooking utensils, which have been obtained by digging; lastly, the well-known habit of the Indians, to celebrate, at the present day, annual funeral solemnities on the places of sepulture; and what Ferdinand de Loto says respecting the way in which the natives of Florida cast up entrenchments, and fortified themselves when their territory was invaded, leaves no doubt as to the nature and origin of these monuments. Only, it is evident that the Indians who raised them formed a more united and numerous nation than the tribes at present remaining; and that the valley of the Ohio was the centre of their operations. The probability is, that, on becoming stronger, they separated; and that, in their exterminating wars, they weakened each other. The very extent of the forts and cemeteries support this conclusion.—*Revue Encyc.* Juillet.

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METEOROLOGICAL DIARY for the Months of June, July, and August, 1829, kept at Earl Spencer's Seat at Althorp, in Northamptonshire.

The Thermometer hangs in a North-eastern Aspect, about five feet from the ground, and a foot from the wall.

For JUNE, 1829.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Monday....	1	43	62	30.00	30.10	NW	WNW
Tuesday ...	2	48	68	30.10	30.10	WNW	WNW
Wednesday.	3	55	76	30.06	30.00	W	W
Thursday ..	4	52	71	29.92	29.83	W	WbN
Friday	5	48	63	29.81	29.91	WbN	NW
Saturday...	6	38	59	30.08	30.08	NW	N
Sunday	7	38	60	30.13	30.19	NNW	NW
Monday....	8	42	63	30.19	30.18	W	NNW
Tuesday ...	9	45	64	30.20	30.17	NE	NE
Wednesday.	10	42	66	30.20	30.22	NE	E
Thursday ..	11	36	71¼	30.22	30.17	E	E
Friday.....	12	43	68	30.17	30.14	E	NE
Saturday...	13	43	74	30.14	30.09	NNE	NW
Sunday	14	63	78	30.05	30.01	W	SW
Monday....	15	52	76½	29.90	29.80	W	SW
Tuesday ...	16	51	63	29.61	29.63	SW	WSW
Wednesday.	17	36	67	29.63	29.66	W	SSE
Thursday ..	18	42	67	29.70	29.79	WNW	NE
Friday	19	44	68	29.83	29.80	S	SbW
Saturday...	20	51	72	29.73	29.64	ESE	EbS
Sunday	21	58	69	29.61	29.61	SE	SE
Monday	22	50	67	29.61	29.62	SE	SE
Tuesday ...	23	51	78	29.68	29.73	SE	SbE
Wednesday.	24	56	75	29.80	29.84	SbE	SSW
Thursday ..	25	45	75	29.88	29.83	SW	WbS
Friday.....	26	51	75	29.74	29.70	W	W
Saturday...	27	54	69	29.51	29.40	E	ENE
Sunday	28	55	61	29.31	29.41	NE	NE
Monday	29	49	64	29.53	29.56	NbW	W
Tuesday ...	30	44	71	29.56	29.56	W	WbS

For JULY, 1829.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Wednesday.	1	50	63	29.48	29.28	SE	SE
Thursday ..	2	48	68	29.30	29.40	SW	SW
Friday	3	48	66	29.43	29.22	SW	SE
Saturday...	4	48	69	29.30	29.29	SW	SW
Sunday	5	43	69	29.43	29.51	WbS	W
Monday....	6	45	70	29.60	29.70	W	W
Tuesday ...	7	49	68	29.70	29.47	SW	SW
Wednesday.	8	50	70	29.60	29.58	WbS	W
Thursday ..	9	47	64	29.58	29.67	W	WNW
Friday.....	10	42	66	29.71	29.56	SW	SSW
Saturday...	11	55	70	29.43	29.30	SE	SE
Sunday	12	52	72½	29.26	29.33	WSW	WbS
Monday....	13	52	71	29.47	29.53	SW	SW
Tuesday ...	14	51	71	29.61	29.61	SE	SW
Wednesday.	15	50	72	29.75	29.77	WSW	W
Thursday ..	16	49	71	29.78	29.75	W	SW
Friday.....	17	47	67	29.75	29.25	S	SE
Saturday...	18	52	71	29.37	29.40	SW	W
Sunday	19	50	67½	29.60	29.70	NW	NW
Monday....	20	47	67	29.83	29.90	WbS	W
Tuesday ...	21	47	72½	29.99	30.01	W	WbS
Wednesday.	22	54	77	30.00	30.00	WSW	W
Thursday ..	23	53	75	30.00	29.94	W	W
Friday	24	46	75	29.90	29.75	W	EbS
Saturday...	25	58	69	29.70	29.75	W	N
Sunday	26	52	64	29.86	29.90	N	NE
Monday....	27	41	65	29.93	29.90	NW	WbN
Tuesday ...	28	51	71	29.90	29.80	W	S
Wednesday.	29	52	67	29.50	29.50	E	SE
Thursday ..	30	41	65	29.58	29.69	S	NE
Friday	31	46	66½	29.82	29.93	NNW	NbW

For AUGUST, 1829.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Saturday ..	1	41½	68	30.03	30.04	W	WbN
Sunday	2	40	70	30.07	30.07	W	W
Monday....	3	48	68	29.92	29.70	SW	SW
Tuesday ...	4	45	62	29.62	29.62	SW	WbS
Wednesday.	5	48	66	29.71	29.86	NW	NW
Thursday ..	6	45	71	29.89	29.90	W	SW
Friday	7	51	74	29.94	30.00	WSW	W
Saturday...	8	53	74	30.03	30.00	W	W
Sunday	9	52	76	29.90	29.80	WbS	SW
Monday	10	54	71	29.73	29.74	W	W
Tuesday ...	11	44	66	29.90	29.98	W	WbN
Wednesday.	12	45	71	29.98	29.98	WbN.	NE
Thursday ..	13	52	72	29.68	29.54	NE	SE
Friday	14	55	67	29.40	29.38	E	NE
Saturday...	15	46	56½	29.43	29.54	NW	NbW
Sunday	16	47	61	29.90	29.99	NW	NW
Monday	17	40	67	29.98	29.90	W	SW
Tuesday ...	18	46	71	29.73	29.58	SSE	SW
Wednesday.	19	52	68	29.37	29.20	SW	SW
Thursday ..	20	48	63	29.11	29.46	NE	NW
Friday	21	45	62	29.61	29.73	W	W
Saturday...	22	43	68	29.75	29.54	SE	S
Sunday	23	50	71	29.29	29.27	S	S
Monday	24	51	60	29.10	29.43	NW	NW
Tuesday ...	25	44	62	29.67	29.80	W	W
Wednesday.	26	38	62	29.88	29.66	W	SbE
Thursday ..	27	44	63	29.37	29.36	WbS	W
Friday	28	49	61	29.33	29.59	NW	N
Saturday...	29	51	64	29.80	29.98	N	N
Sunday	30	46	66	30.00	30.00	N	N
Monday....	31	47	65	30.00	29.92	NW	NW,

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ALBEMARLE STREET.

PLAN

OF AN EXTENDED AND PRACTICAL COURSE OF LECTURES AND DEMONSTRATIONS ON

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DELIVERED IN THE LABORATORY OF THE ROYAL INSTITUTION,

BY WILLIAM THOMAS BRANDE, F.R.S.

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AND

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§ 3. Electricity—Its Laws and Connexion with Chemical Phenomena.

§ 4. Radiant Matter.

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OF UNDECOMPOUNDED SUBSTANCES, AND THEIR MUTUAL COMBINATIONS.

Substances that support Combustion: Oxy-
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Acidifiable Substances;
Sulphur—Selenium—

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§ 2. Modes of Analysis—Ultimate and Proximate Elements.

§ 3. Processes of Fermentation, and their Products.

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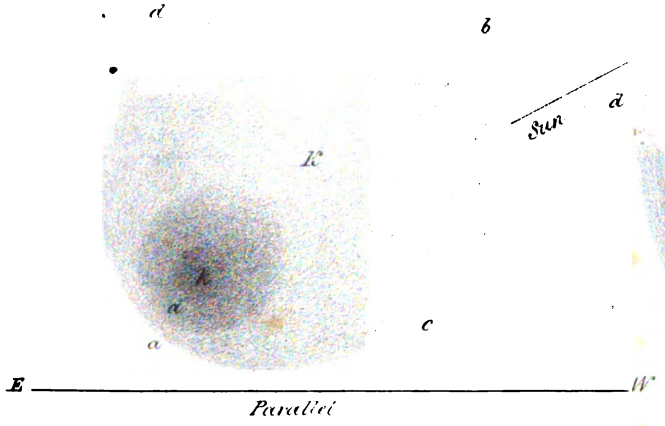
Fig. 2. 30. Nov.r



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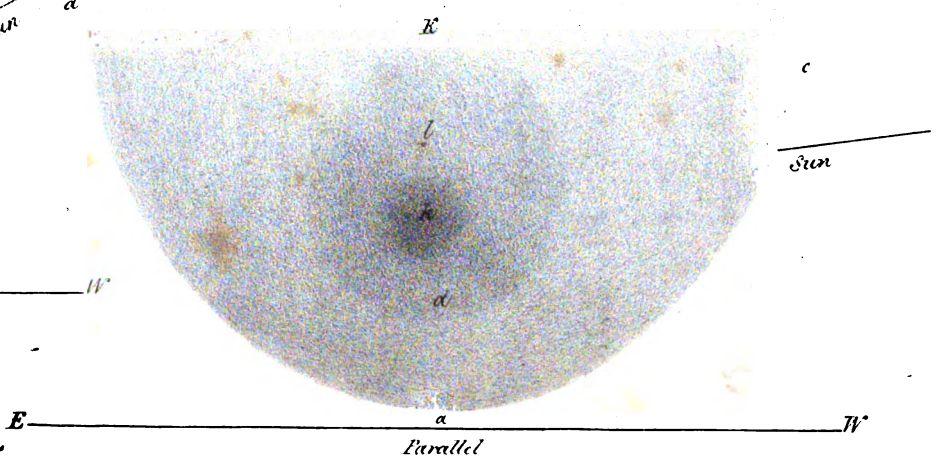

Published by Longman, Hurst, Rees & Packard, Paternoster Row in the Quarterly Journal of Science, Jan.y 1.st 1830.

Printed by C. Hullmandel.

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THE

QUARTERLY JOURNAL

OF

SCIENCE, LITERATURE, AND ART.

Observations on the Relations which exist between the Force, Construction, and Sailing Qualities of Ships of the Line.

With a former number of the *Journal of Science* *, we gave a brief disquisition on the *force* of ships-of-war; we shall now prosecute our inquiries, and endeavour to shew in what manner the force influences and connects itself with the theoretical construction and sea-going properties of such vessels.

In the construction of a ship-of-war, it becomes a matter of imperious necessity to consider the relation between its military and sailing qualities; for it is to very little purpose that the ship possesses the former, if it have not the latter to take them into action and to render them fully available and effective.

As the limits of a single memoir preclude us from entering into the proposed discussion relatively to all classes of ships-of-war, we shall at present confine our inquiries to those which are denominated *ships of the line*, reserving for a future paper a similar task in relation to frigates and smaller vessels.

The easy service of a piece of artillery, as we have already seen in our former paper, is dependent on its weight and calibre, the *maximum* of which, for sea service, we have therein endeavoured to indicate, from the examination of some facts with which we have been furnished from the practice of our own and foreign naval artillerists. But, however true this maxim is, with regard to a *single* gun, it requires to be modified when another gun is placed on each side; for the juxta-position

* No. VII. of the New Series.

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occasioned thereby requires us to obtain such an interval between them, that the gunners, when at quarters, should not be crowded. When this precaution is neglected, much confusion and many accidents must arise from not having sufficient room to handle the tackling and various implements necessary to the service of a heavy piece of ship ordnance.

In land batteries it is the usual practice to allow a distance of 18 feet between the centres of the embrasures; but on board ship, wherein it is desirable to concentrate as much force as possible, it is common in the British navy to give no greater distance than 11½ feet between the centres of the lower ports of ships of the line. Now, when it is considered that the guns are frequently trained obliquely, whereby this space becomes much contracted, we may regard 11½ feet as the *least* distance that can be allowed from port centre to port centre of the heaviest tier of guns, in order that they may be efficiently served *.

Having this *datum*, we are enabled to determine what length of broadside is necessary to put a given number of guns into the lower battery of a ship of the line; and here we immediately perceive that the quantity of ordnance on the lower deck indicates the *minimum length* of the ship of whose armament it forms a part.

It may next be remarked that the guns also prescribe the *least* limit to the breadth of the ship; for, measuring from the hatches and ladder-ways on the lower-deck, a sufficient distance should be left between them and the rear of the gun-carriages, after recoil has taken place, for a clear communication fore and aft. The increased dimensions of modern ships have caused this limit to be much exceeded. The only part therefore of the half-breadth of the ship at the lower deck that has suffered any material increase in consequence, is that just specified, viz., the distance between the rear of the carriage after recoil, and the hatchway. In the old 80-gun ships on three decks, this

* If this distance were somewhat increased, it would be highly advantageous, not only by giving more room for the gunners, but also by increasing the dimensions, and obtaining a better ship. Such a proceeding may be objected to on the score of economy; but we contend that the most *economical* ship is that which is the *most effective*.

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space was not 6 feet*; in the present 84 it is about 9½ feet, and in the 120 about 10½ feet.

Having thus seen how the force indicates the *least* limits of the simple dimensions of length and breadth, we have now to observe that the *depth* in ships of the line is a fixed dimension in comparison with the other two. Nevertheless, whilst the *minima* of the former are *directly* dependent upon, and indicated by the *force*, the *minimum* of the latter is likewise *indirectly* affected by the same element; for the space in the hold, which involves all three dimensions, must be sufficient for the stowage of the ballast, and the necessary quantity of water, provisions, and stores of every description, all which are ultimately referrible to the number and calibre of the ordnance. The *greatest* limit of the depth is pointed out by the draught of water in our roadsteads and ports.

As all the sailing qualities of a vessel are influenced in some way or other by the three principal dimensions and their mutual relations, we ought, after having determined on the *force*, to consider in the next place in what way it may be made to bring along with it an excellent ship. This naturally implies, that such an adjustment must be made of the ordnance as shall produce those relations between the dimensions which have been found conducive to the qualities essential to a ship-of-war, and particularly the velocity, the non-attainment of which more than counterbalances every other excellence. Instances are not wanting, even in the history of modern naval architecture, of a bad adjustment of the force being the ultimate cause of throwing *whole classes* of ships into disuse, by producing badly proportioned, slow sailing, and inefficient vessels. It is sufficient to mention the abolition of the 80-gun ships, on three, and 64's, on two decks, in corroboration of our assertion.

It must be confessed, that the connection between the principal dimensions of a ship-of-war and its force, has rarely, if ever, been understood, or duly appreciated in this country. Indeed, so little consequence has been attached to it, that even the ships composing the very recent experimental squadrons were ordered to be of a *certain tonnage*; thus at once

* This deduction is made with a gun 9½ feet long, the common length for a 32-pounder.

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fettering the efforts of the constructors, by a restriction which, of all others, is perhaps the most calculated to neutralize attempts at improvement. The present absurd method of estimating the tonnage of a vessel can only be tolerated on the plea of being a kind of standard by which ships may be bought and sold, and which, from long usage, being understood as such, it would perhaps be productive of some inconvenience to abolish; but that it should ever form a *datum* for the *construction* of a ship of war, cannot be too strongly condemned; because, from the manner in which it is obtained, there can be no permanent relation between it and the *real tonnage;* that is, *the difference between the weight of the hull and the total weight of the ship when fully equipped and ready for service.* To attempt the construction of a ship intended for the purposes of war, without referring the design to the quantity and calibre of artillery it is to bear, is a method of proceeding which, abandoning data at the very outset, involves the whole problem in distracting confusion, and inevitably concludes by leaving the solution to chance.

When we attempt the construction of a ship of the line, it should be remembered that there are three ways of distributing the quantity of ordnance it is to be armed with;—

1st. The length of the ship being given, we may vary the number of decks on which guns are to be mounted.

2dly. The number of decks being restricted, we may augment or diminish the number of cannon carried on each.

3dly. The combination of the two foregoing methods also furnishes means, by which the force of a ship of the line may be adjusted, so as to suit the views of the constructor in other respects.

By adopting the first mentioned of these methods, we increase or diminish the height of the hull above water; and by resorting to the second, the length of the ship will be similarly affected. The third method we have noticed, will combine the effects of the two first, viz. an increase or decrease, both in the number of decks, and in the number of guns carried in each separate tier.

As the first method of adjustment can never be used without deteriorating all the most desirable properties of a ship, considered as a marine loco-motive machine, it is never resorted to: the same objections, however, do not exist against the second, nor to the third expedients when judiciously employed; and

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they afford ample latitude to the naval architect for making such a disposition of his force as shall produce the best sea-going qualities.

Since the introduction of cannon on board ship, towards the end of the fourteenth century, the progress of improvement has produced great changes in the disposition of the ordnance, and consequently in the relative proportions and sizes of vessels of war. From first carrying guns to fire *over* the gunwale, then through holes pierced in the breastwork of the weather-deck; the rivalry, which soon sprang up between the maritime powers of Europe, produced, at the beginning of the sixteenth century, ships of two entire tiers of ordnance*, but of very small dimensions, and in accordance with the then existing prejudices, having extravagantly lofty hulls. The celebrated Sovereign of the Seas, of 100-guns, built by Phineas Pett, in 1637, seems to have been the earliest ship of *three* whole tiers of cannon, that we have any distinct records of, and was at that time a great effort to combine force with a lofty hull. From the then very imperfect state of naval science, and a great, if not total absence of data, to guide its constructor, the attempt was a very hazardous one, and nothing but the bold increase of dimensions, which the science of Pett gave to this ship, rendered it at all successful†. The prepossession which, for such a length of time, existed in favour of these towering but inefficient ships, at length gave way to a more enlightened practice with all the maritime powers, excepting the Dutch‡. In this country the bold views of Pett died with him, and our naval architects relapsed into their old methods, from the perversity which usually attends ignorance, and caused the English navy not merely to stand still, as far as regarded the

* The contrivance of port-holes, by which a ship is rendered capable of using artillery between the gunwale and the water, took place in the year 1500, and is said to have proceeded from Descharges,—a French shipbuilder at Brest. The Harry Grace de Dieu, built in 1515, exhibits a very early adoption of this simple but important idea.

† Mr. Knowles, in some very able lectures on Naval Architecture, delivered in May, 1828, at the Royal Institution, tells us that the length of this ship was 167.75 feet, and breadth 48.33 feet, or about the length of the old Bellerophon of 74 guns, built in 1786, with a foot more breadth.

‡ It would appear from Pett's ships, that he had made some attempts towards suppressing the enormous and unnecessary weight of hull above water. Raleigh had, however, pointed out the necessity of doing this long before.

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improvement of its ships, but absolutely to retrograde. They contented themselves with building lofty hulled vessels, which, from their small dimensions, were incapable of carrying their ordnance and stores without their lower ports being almost under water, besides being generally deficient in every requisite; and it was not until the middle of the last century that the superior qualities of the French and Spanish ships forced the constructors of our navy to begin a reluctant imitation of foreign models.

This humiliating proof of our inferiority, as naval constructors, was the natural consequence of the widely-different conduct of the governments of France and this country, in their treatment of science. Whilst the French Academy of Sciences, under the auspices of Louis XIV. and his successors, were holding out every incitement for men of genius and learning, to cultivate the mathematical principles of naval construction, the matter in this country was wholly neglected. The desire of that Monarch to obtain a maritime preponderance for his country, and the encouragement he wisely held out to men of science, may be said to have originated a new era in this noble art. The physico-mathematical laws of floating bodies, which, from the time of Archimedes till then, had lain dormant, began to be applied to ships; and the gradual developement of the fundamental principles of naval construction, by some of the most eminent mathematicians of the eighteenth century, gave rise to ships of the line of greatly enlarged dimensions, and superior qualities.

The many failures which took place, before and after Pett's time, in the construction of English ships of the line, proceeded from our naval architects not knowing how to appreciate, and contend with certain difficulties which so much top weight produces: difficulties, which, in a ship of more than two decks, rapidly increase. The superior knowledge of the French constructors enabled them at an early period of their efforts to triumph over these obstacles in their two-decked ships of the line; but they did not meet with equal success in their three-decked ships of 90 and 104 guns, although they were much superior to our own in qualities. The difficulty, indeed, of producing a first-rate ship of commensurate excellence appeared so insurmountable, that they were induced,

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after the peace of Utrecht, to discontinue their construction, and confine their ships of the line to those of two decks, mounting 74 and 84 guns. The constant patronage, however, given by the French government to its naval architects, amongst whom were many excellent mathematicians, was at length rewarded by seeing its fleets, in the war which broke out in 1778, again reinforced by three-decked ships of the line of superior force, and endowed with every desirable property, in almost as eminent a degree as those of two decks; and a further improvement in 1786, which increased the force to 120 guns, and carried the length to 209 feet, may be said to have completed the triumph of the French constructors. At this time, and until 1796, the largest three-decker in the English navy was the Victory, of 100 guns, and 186 feet long. In 1796, the Ville de Paris, carrying 110 guns, and 190 feet long, was built at Chatham; and the battle of St. Vincent, in 1797, put into our possession the San Josef, of 112 guns, which was, for many years, the largest and finest ship in the fleet.

The Spanish navy, at this period, possessed several ships of equal size and excellence with the San Josef. The almost uninterrupted alliance of Spain and France during the eighteenth century, and the consequent introduction of science into the Spanish dockyards, ultimately produced similar effects on the naval architecture of these two nations. One splendid proof* of the science of the French naval architects fell into our hands by the capitulation of Toulon in 1792, and gave rise to the increase of dimensions which has so much improved our three-decked ships of the line; but it was not till 1809, that the British navy could boast of the Caledonia, of 120 guns, and 205 feet long. But, although modern science has achieved such a victory, it cannot be said to have made any successful attempt beyond it as far regards the *number of decks*, which, two centuries ago, had reached its *present limits*. We cannot cite the instance that has occurred in the Spanish navy, of an efficient *four*-decked ship, in contradiction to this statement, because it had not the merit of being originally built as such†, and could not have undergone the alteration

* The Commerce de Marseilles.

† The following description of this famous ship is given by James, in his Naval History, vol. ii. p. 66:—

"The Santissima Trinidada was built at Havana, in 1769, as a 112-gun

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from its previous state, as a three-decker, without detriment to its sailing qualities.

Thinking that it is very improbable that ships of the line, of more than three decks, will ever become common, if even attempted, through physical causes, we have to remark, that there remains now only one way of increasing the force of a ship of the line, the calibre of its artillery being given, and that is, by making use of the second adjustment of the ordnance we have mentioned. Actual trial has not yet pointed out where it ceases to produce improvement. The American line-of-battle ships of two decks are, for the most part, 206 feet long, and mount 102 guns of the calibre of forty-two and thirty-two pounds*: and they have ventured on a three-decker of at least 220 feet length, and corresponding force. The French are running, with vigour, the prosecution of maxims which originally emanated from them, and are said to be constructing a first-rate of 232 English feet in length†. These enlarged two-decked ships have been proved in actual service by the Americans, and found to answer the expectation formed of them. Whether the enlargement of the three-deckers exceeds propriety or not, remains to be determined. For our own parts we have very little, if any, doubt, as to the successful issue of this important and interesting experiment; and if a proper modification be made in the masting and canvas, there will not be any necessity for a size of masts and yards beyond what nature allows in sufficient plenty to our wants in this case.

Having endeavoured to shew the influence which the force of a ship of the line exercises over the principal dimensions,

ship, similar to the San Josef, or Salvador del Mundo, except, probably, in possessing rather more breadth of beam. It appears that, some time between the commencement of 1793 and 1796, her quarter-deck and forecastle were formed into a whole deck, barricades built up along the gangways, and ports cut through them, so as to make the total number of 8-prs. on that deck equal, in amount, to the 12-prs. on the deck next below it. This accounts for 126 guns: the remaining four, we may suppose, were mounted on the poop. The Santissima Trinidada was, therefore, a flush four-decker that exceeded the three-decked 112's in force only by fourteen 8-prs., and four pieces of a still smaller calibre."

* The North Carolina, one of these ships, discharges at a broadside 1972 pounds of iron, whilst our Caledonia can only project 1568 pounds.

† A length of 232 feet would allow nineteen ports of a side on the lower battery.

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we shall now proceed to inquire how far the *displacement* is affected by the same *datum*.

It is, or ought to be, an object of great solicitude, when the design of a ship-of-war is formed, to make its whole force available and effective, not merely in still water, but also when the sea is much agitated, and the power of the wind on the sails is such, as to produce a considerable inclination from the upright. Under the latter circumstances, if the lower ports be so depressed as to be below the waves, we lose the services of the lower battery in action, or at least fight its guns with very great inconvenience and liability to disaster. To prevent such bad consequences, it is absolutely necessary, in the construction, to give to the ports a due elevation above the assumed plane of floatation, and, with this plane as a base, to obtain a displacement, whose buoyancy shall be equal to the weight of the ship when ready for sea. If this precaution be not attended to in the first instance, the constructor must inevitably labour under all the anxieties attending a distracting uncertainty and a fearful looking for of failure in a vast and expensive structure. Every attempt at an after adjustment, in this particular, will produce not only a deterioration of some other important quality, but a general vitiation of the design; and nothing but a complete acquaintance with the *rationale* of his art can enable him to proceed with confidence and precision. It is true, that in the present state of science, the physico-mathematical laws of naval construction cannot all be derived *à priori;* but those which cannot, may be developed by the *analysis* of ships. The inductive method must naturally be disagreeable to those who are in the habit of mistaking self-confidence and fancies for real knowledge and well-founded deductions; but it is the only one capable, from its accumulation of data, of conducting us with certainty in the path of improvement; and, with the corps of naval engineers which this country now possesses, it is to be very much regretted that the energies of some of them are not, under *proper authority*, directed to the *permanent* prosecution of an undertaking so pregnant with advantage, as the analysis of the ships of the British navy. We have seen quite enough of the blind efforts of ignorant mystics and pretenders to be fully satisfied of the impotency of quackery, and to lament, that our leading naval

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authorities should be so much exposed to its deceptions, for want of a scientific reference of this description and its legitimate deductions.

A ship of the line to be fully effective in action, when the weather is rough, should not, in *any* case, have the lower port-sills a less height above the water's *level* surface than 5 feet 9 inches, or 6 feet; and in one of *two* decks this height may be increased to 6½ or 7 feet without materially compromising other good qualities. There is, no doubt, some difficulty in effecting a greater height than 6 feet in ships of three decks, and they have already so much hull above water, that it becomes an object to prevent a further increase; but the same objection exists only in a far inferior degree in a ship of *two* decks; and as the failure of its lower battery would put it out of the line of battle, and, with the usual armament, would render it inferior in force to the heavy frigates of the present day, it becomes a matter of still graver importance to secure this class of ships of the line from losing the fire of their lower tier of ordnance.

The weight of the volume of water displaced by a ship when fully equipped for service, is susceptible of being divided into two principal parts, viz., the weight of the hull, and the weight of the equipment, in which we include the masting and ballast. The first-mentioned of these weights may be considerably varied from a difference in the practical carpentry, and in the specific gravity of the timber material used; but the second being that incidental to the armament, or such as is positively incurred by the adoption of a certain force, must bear a constant ratio, or nearly so, to that force, in ships of the same nation. What variation there may exist, must consist principally in the alteration of the stores and crew, from the war to the peace establishment, or from home to foreign service proportions. As, however, the good qualities of a ship-of-war are most especially valuable and desirable in belligerent times and on foreign stations, we should prefer constructing the best ship to meet such circumstances, and at the same time endeavour to preserve the *permanence* of its good qualities as much as the gradual consumption of water, provisions, and stores will allow.

The two following Tables will indicate very nearly the general

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state of naval construction in England, France, and Sweden. They give a comparative view of the force, and of some of the most important particulars which present themselves at the outset of a design for a ship of the line. We do not hesitate to say, that without a knowledge of these data, it is impossible to proceed with any degree of certainty, or to possess any control over the results of our operations. It is proper to observe, that the elements of the English ships given in the second table, have been calculated from the draughts of water and height of lower port-sills given in official returns. The 120-gun ship, here partly analysed, is of the Caledonia class; the 84, of the Canopus, or French Franklin, class; the ship of 76 guns is the Bulwark; and that of 74 is the Blenheim or Ajax, similar to which about 40 ships have been built. The elements of the French ships are mostly derived from the *Ordonnance de la Marine* of 1786; and those of the Swedish from the superb work of Chapman, published in 1806 *. The weights and measures have been carefully reduced to those of England †.

* It is much to be lamented that this work of Chapman is, as it were, a sealed book to the English constructor, from its scarcity, great expense, and being in a foreign language very little known in this country. It is replete with valuable data and information with regard to ships-of-war, and it would redound greatly to the honour of our Admiralty to have it promulged in our dockyards in an English dress, and to order it to be used as a text-book in our Naval Architectural School at Portsmouth, which, it must be confessed, is at present much in want of such a well arranged and useful treatise.

† It will be perceived from the first table, that Chapman tasks the manual power of Swedish seamen at a much higher rate than even the French service does, and he says unequivocally, that their strength is fully equivalent to it. We presume, therefore, that he reckons on the superiority of *his* gun-carriage for facilitating its service. In our former paper we have given an exposition of our views with regard to increasing the powers of our sea service ordnance, and have detected the fallacy of the arguments on which our maximum calibre, the 42-pr., was discarded. We now feel additional confidence in the truth of what we have before stated as to the propriety of restoring that powerful gun to our navy, not only from what Chapman has advanced, but also from having before us Commander Marshall's very recent work, developing a system of mounting naval ordnance, at once novel, simple and efficacious. No one, we presume, can deny its originality; and actual trial at sea for the last two or three years has fully determined its efficacy. We shall at an early opportunity endeavour to take a detailed view of Commander Marshall's principle, which is calculated to produce the most important ameliorations in a system of mounting sea service guns, that has continued from the times of the Tudors to the present day with scarcely an improvement.

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TABLE I., *exhibiting the Comparative Force of English, French, and Swedish Ships of the Line.*

Navy belonging to	Total Number of Guns.	Lower Deck.			Middle Deck.			Upper Deck.			Quarter-Deck and Forecastle.			Weight projected at one broadside, in pounds avoir.
		Number of Guns.	Calibre.	Calibre, in pounds avoir.	Number of Guns.	Calibre.	Calibre, in pounds avoir.	Number of Guns.	Calibre.	Calibre, in pounds avoir.	Number of Guns.	Calibre.	Calibre, in pounds avoir.	
English	120	32	32	32.00	34	24	24.00	34 Congreve.	24	24	8 12 Car.	12 32	12.00 32.00	1568.00
French.....	120	32	36	38.86	34	24	25.92	34	12	12.96	8 12 Car.	8 36	8.64 38.86	1770.76
Swedish ...	110	30	48	55.58	32	42	48.62	30	24	27.73	18	12	13.84	2152.13
English	84	28 2	32 68 Car.	32.00 68.00	...	...	.....	32	24	24.00	8 Congreve. 14 Car.	24 32	24.00 32.00	1220.00
French.....	80	30	36	38.86	...	...	.....	32	24	25.92	6 12 Car.	12 36	12.96 38.86	1269.66*
Swedish ...	80	30	42	48.62	...	...	.....	32	24	27.73	18	12	13.84	1297.54†
English	76	28	32	32.00	...	...	.....	30	24	24.00	6 12 Car.	12 32	32.00 12.00	1018.00
English	74	28	32	32.00	...	...	.....	28	18	18.00	6 12 Car.	12 32	12.00 32.00	928.00
French.....	74	28	36	38.86	...	...	.....	30	18	19.43	6 10 Car.	8 36	8.64 38.86	1055.76
Swedish ...	74	28	36	41.69	...	...	.....	30	24	27.73	16	12	13.84	1110.33

* If this ship were mounted with two more 12-pounders, and two more 36-pounder carronades, the weight of the broadside would be increased to 1320.82 pounds avoir.
† If the armament of this ship were augmented by four more 12-pounders, the weight of the broadside would be increased to 1324.68 pounds avoir.

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The weight of the 48-pounder proposed by Chapman is	84·36	cwt.,	or 170	times the weight of the shot; and is 9 feet 9 inches long, English measure.	
The weight of his 42-pounder is . . . and is 9 feet 4½ inches, English, long.	74·23	„	171	„	„
The weight of the common 36-pounder, Swedish, is and is 9 feet, English, long . . .	65·52	„	176	„	„
Ditto, Ditto, 30-pounder, ditto, is and is 8 feet 7 inches, English, long . .	55·42	„	178	„	„
Ditto, Ditto, 24-pounder, ditto, is and is 7 feet 9 inches, English, long . .	45·17	„	182	„	„
The weight of the French 36-pounder, is . . and is 10 feet 1½ inches, English, long . .	71·00	„	207	„	„
Ditto, Ditto 24-pounder, is . . and is 9 feet 7 inches, English, long . .	52·00	„	202	„	„

In our former paper the Swedish 36-pound shot was erroneously stated at being equivalent to 33·73 pounds avoirdupois. This mistake arose from regarding the calibre as expressed in common Swedish pounds, instead of the pounds in which the Swedes express the calibre of their guns.

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Table II., *giving a Comparative View of some of the primary Elements of Naval Construction derived from English, French, and Swedish Ships of the Line, when equipped and stored for foreign service, in time of war.*

Of what construction.	No. of Guns.	No. of Crew.	No. of Months.		Length in feet to outside of wales at the water section's middle line.	Breadth in feet to outside of the wales at the water section.	Draft of water in feet.		Height of Midship lower port above the water, in feet.	Tons per inch submersion, with water section as a base.	Displacement in tons.	Tons displacement per gun.	Weight of hull, in tons.	Real tonnage.	Real tonnage per gun.	Ballast, in tons.	Real tonnage, exclusive of ballast.	Real tonnage per gun, exclusive of ballast.
			Water	Provisions.			Forward.	Abaft.										
English ..	120	875	3	6	205	54·41	25·02	26·77	4·61	23·50	4841	40·34	2532	2309	19·24	326	1983	16·52
French ...	120	1098	3	6	209	55·32	24·74	27·17	5·78*	25·00*	5010*	41·75	2327	2683	22·35	622	2061	17·17
Swedish ..	110	1000	2·7	5·4	204	56·34	22·96	24·91	6·31	24·05	4251	38·85	1828	2423	22·23	483	1940	17·63
English...	84	700	3	6	196	52·41	23·20	25·78	5·30	20·50	3806	45·31	1915	1891	22·51	240	1651	19·65
French ...	80	839	3	6	194	52·41	22·82	24·42	5·86	20·00	3687	46·09	1651	2036	25·45	489	1547	19·34
Swedish ..	80	706	3	6	183	51·09	20·85	22·64	6·74	19·43	2986	36·59	1469	1517	18·22	296	1221	15·26
English...	76	650	3	6	182	49·75	22·30	23·30	4·89	18·73	3322	43·71	1838	1484	19·52	220	1264	16·63
English...	74	600	3	6	176	47·90	22·05	24·10	5.16	17·63	3060	41·35	1690	1370	18·51	174	1196	16·16
French ...	74	690	3	6	183	48·77	21·14	22·92	5.86	18·29	2938	39·71	1307	1631	22·04	331	1300	17·54
Swedish ..	74	658	3	6	177	49·71	20·21	21·99	6·65	18·23	2681	36·43	1368	1313	17·90	256	1057	14·28

* These Elements have been obtained by calculations on the draft of the Commerce de Marseilles.

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From the last Table may be derived the following one, in which these ships are, with respect to the height of lower ports above water and the quantity of displacement, referred to the same standard of comparison, viz., the real tonnage of the English ships of the same number of guns; or, in other words, the whole of the foreign vessels are supposed to be equipped for foreign service, in time of war, as if they belonged to the English navy; ten guns more being mounted in the Swedish three-decker, and four in the French and Swedish 80-gun ships.

TABLE III.

Of what Construction.	No. of Guns	No. of months.		Height of Midship Port above water, in feet.	Displacement, in tons.	Displacement per Gun, in tons	Real tonnage.
		Water.	Provisions.				
English	120	3	6	4·61	4841	40·34	
French	120	3	6	7·02	4636	38·63	2309
Swedish....	120	3	6	6·68	4137	34·47	
English.....	84	3	6	5·30	3806	45·31	
French	84	3	6	6·45	3542	42·16	1891
Swedish....	84	3	6	5·03	3360	40 00	
English	74	3	6	5·14	3060	41·35	
French	74	3	6	7·01	2677	36·17	1370
Swedish	74	3	6	6·38	2738	37·00	

(To be continued.)

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On Siliceous Gravel. By Robert Vaughan, M.B., St. Mary Hall, Oxford.

Some foreign writers speak of urinary concretions consisting of *siliceous matter*, in sufficient quantity to establish the character of the species; and Berzelius, in his treatise on the blow-pipe, gives a formula for their discrimination*. He even points out the characters distinguishing this class from those of the phosphate of lime†. From these circumstances, it might probably be inferred, that siliceous deposits from the urine were by no means of very rare occurrence. It does not, however, appear that any of the British practitioners, who have been most conversant with urinary disorders, have observed them either as gravel, or as forming any sensible portion of urinary concretions. Dr. Marcet has not mentioned its existence in any one of the many calculi which he analyzed, and if it had existed it could not have escaped so close and so accurate an observer. Dr. Wollaston, I believe, has not met with it either; and Mr. Brande, in his Manual, does not speak of it as constituting any portion of urinary calculi. Of 328 calculi—part of the collection of the Norwich Hospital—analyzed by Dr. Yellowley‡, there is not one stated to contain the smallest proportion of siliceous matter. Hence then, it would appear, that in this country, at least, urinary siliceous concretions are extremely rare, and that, as yet, there has not been an instance recorded§.

* "Alone," he says, "they leave an infusible scoriaceous ash, which fuses with a small quantity of soda, slowly, and with effervescence, into a more or less transparent glass globule." Berzelius on the Blow-pipe, translated by Children, p. 332.

† "A proof that these calculi (phosphate of lime) are not siliceous, is, that they swell up with soda without vitrifying, and when dissolved in boracic acid, and then fused with a little iron, they give a regulus of phosphuret of iron." Ibid., pp. 330, 331.

‡ Philosophical Transactions, 1829, part 1st.

§ Of 600 calculi, analyzed with great care and exactness, by Fourcroy and Vauquelin, only two were found to contain silica. Professor Wurzer states, that he detected silica to the amount of one per cent. in a calculus analyzed by him. Its composition he states to have been as follows:—

Lithic acid	75·34
Phosphate of lime	17·33
Animal matter	6·33
Silica	1·00
	100·00

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On the contrary, the highest authority on the subject in this country, is decidedly against the opinion that silex does form any part of urinary gravel. Thus Dr. Prout, in his very valuable Treatise on Urinary Disorders, observes, "Silex has been stated to constitute urinary sediments, and even to form a part of urinary calculi in some instances; but this assertion requires to be better authenticated than it is at present, before it can deserve credit*." Indeed, Dr. Prout communicated to me, personally, his doubts as to the existence of silex, either as gravel or calculus, and expressed his desire of having the point satisfactorily decided. Certainly Berzelius assents, that it ordinarily exists in the urine, in minute quantity, and that it is probably derived from the water which we drink; but the foregoing facts tend to prove, that in this country, at least, silex is not apt to appear either as gravel or in the form of urinary calculi.

I have, however, met with two instances, and which, as affording matter interesting no less in a scientific than in a professional point of view, I deem of sufficient moment to claim attention. From the nature of the facts and the questions which they involve, I have preferred the *Quarterly Journal of Science* as the most eligible medium of communication.

The first case occurred in the practice of my friend, Mr. Henry Bird, surgeon of this town†, and was that of a woman thirty-two years of age, married, having two children living, rather unhealthy, the disordered tendencies being to pulmonic irritation. She has been pregnant, in all, seven times, including two miscarriages; of the five children born alive, three died and two only now survive. The unfortunate sufferer herself presents every indication of bad health and a broken-down constitution—emaciation, languid, anxious countenance, hectic flush, foul sulcated tongue, hot feverish state of skin, though, at times, subject to perspirations, frequent wiry pulse,

Allemain, an Italian apothecary, states that he found silica in excessive proportion—20 per cent.—in a calculus which he analyzed; but as he has given no detail of his mode of analysis, some doubts may be entertained as to the correctness of the results.—*Vid. Ann. de Chimie*, xxxii. p. 221, and xlv. p. 222, and *Gehlen. Journ.*, second series, ii. p. 265.

* Prout, on Calculus, &c. pp. 28, 29, second edition.

† Chelmsford, Essex.

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with dyspnœa. When she consulted Mr. Bird, she complained most of the pulmonary affection; but after the urgency of the symptoms had been subdued by venesection and other appropriate means, she was seized with severe pains in the back and loins. They were of a sharp lancinating description, and shot, as she termed it, from the kidney to the bladder, observing the exact course of the ureter. These pains continuing to increase, and becoming every day more distressing, Mr. Bird requested me to see her, which I did on the 20th May, 1828. I found her in the state above described, and with other symptoms of severe urinary disorder. She told me that she had for a long time considered herself subject to gravel. A few days after I visited her, she gave to Mr. Bird two small concretions passed at separate times, and with a short interval between. Mr. Bird handed me one, but the other he unfortunately lost, and he had not attended to its characters. That which I received presented the following characters:—It was about the size of a large pin-head, rather opaque, of a whitish colour, and of an oblong shape, slightly excavated. The shape very much resembled that of a cystic oxide calculus in my possession, and which was passed by a woman in this neighbourhood; the details of which I shall endeavour to communicate in a future Number. This figure I consider as bearing some slight resemblance to that of the kidney itself. Exposed to the flame of the blowpipe, it underwent no change whatever, although it was urged for upwards of ten minutes, and exposed during this time to the strongest heat I could excite with this instrument. It scratched glass like common flint. Neither acetic, nitric, nor muriatic acids, though boiled upon it, produced any effect. It was then treated with caustic potass, but without any sensible effect. Having occasion to go to London, I took it and shewed it to my friend Dr. Prout: he, at first, thought it was a tooth, but, on looking at it more closely, he immediately pronounced it silex, after writing with it upon one of the panes of glass in the window of his study. I then detailed to Dr. Prout its history, and it was upon this occasion that he told me his doubts as to the existence of urinary siliceous concretions, and expressed his desire of having the question satisfactorily and unequivocally decided. Lastly, on my return to

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Chelmsford, I again submitted it, under a less objectionable form, to the action of the re-agents already mentioned and with similar results. I then mixed it with soda, and exposed it to the flame of the blowpipe, when it melted slowly, and at last fused into a semi-vitreous globule, with scarcely any degree of transparency. These refractory characters place the nature of this substance beyond any doubt. Being in the habit for the last fourteen or fifteen years of examining the general properties of the urine in all cases, where practicable, in which I am concerned, and having never met with any similar production, I felt doubtful as to the urinary origin of this substance. I have not the slightest suspicion of any attempt at imposition * on the part of this poor woman; but the siliceous substance might have been in the utensil, and the patient seeing it, after having voided the urine, might have imagined that it passed from the bladder with this fluid. This makes the loss of the other little calculus, which was passed at a day or two's interval, the more to be regretted, as its nature would have tended, in some degree, to clear up the mystery. I was not, therefore, inclined to attach much importance to this single instance, more especially as its urinary origin was not unequivocally proved. But having lately met with another instance in which siliceous matter, in the form of very minute angular grains, has been passed with the urine, I can no longer doubt the circumstance, and have therefore determined to present the facts to the scientific part of the profession.

The patient, in this instance, is also a married woman, thirty-four years of age, of very delicate constitution, and, in general, bad health. She has also, for a long time †, suffered with symptoms of urinary derangement, and has frequently passed blood with the urine. There is also a considerable degree of uterine disease, as, although she has been several times pregnant, she has never given birth to a living child, nor gone her full time; yet her mother has had fifteen

* Dr. Marcet states, that patients frequently attempt to impose upon medical men, without any ostensible motive. Could he have met with instances of the above description, and considered them as attempts to deceive and impose upon the practitioner?

† She states since 1819.

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children. She first consulted me on the 22d of October, 1829, complaining of very severe and excruciating pains in the back and loins. The pain also passes along the course of the ureter from the kidney to the bladder. She now, for the first time, perceived a reddish-looking sand at the bottom of the vessel (a tea-cup), in which the water was made. The urine was extremely scanty, not making much above half a tea-cup full at several times. She brought the specimen with her, and on looking at the bottom of the cup, I saw a very small quantity (about ·8 of a grain) of a reddish-looking sand, which I took for lithic acid, the minute crystals of which it very closely resembled. Upon this conviction I prescribed for her, but desiring her to leave the vessel with its contents, till a convenient opportunity for examining them. The urine had been passed in the morning about an hour or two before it was brought to me, and, in the course of the day, I commenced the examination. Having poured the urine off, the sand was found collected together in a small mass at the bottom of the cup. A small quantity being placed upon charcoal, and urged with the blowpipe, presented a scoriaceous appearance, without consuming or leaving the white ash usually left by lithic acid. Reddened litmus paper, being moistened and brought in contact with it, indicated no alkaline re-agency. A small quantity was now placed in a small glass capsule, and a drop of nitric acid being placed upon it, it was heated. An effervescence took place, but on continuing the heat till the acid was dissipated, the dried mass presented nearly the same appearance as before the experiment. On being exposed to the vapour of ammonia, there was a slight indication of the presence of lithic acid interspersed through the mass. I now examined it with a small magnifying glass, and perceived that it consisted of small angular crystals, transparent, some colourless like quartz, others of an ambery or topaz colour, and having a certain degree of refractive density. On treating them with caustic potass *, and then washing them, nearly the

* Silica is soluble in the caustic fixed alkalies; but it is not very sensibly affected by the liq. potassæ of the shops, unless the silica be in a state of minute mechanical division, and after a prolonged digestion. The lithic acid, on the contrary, is very speedily taken up by this

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whole of the red colour was removed, and the mass became nearly colourless. They resembled the very fine crystals which we frequently see in sand or gravel upon the sea-side. On taking a small crystal, and pressing it with my nail, and drawing it over a piece of window glass, it scratched it like common flint. Mixed with soda, and heated with the blowpipe upon charcoal, they melted slowly, and with effervescence, into a vitreous globule. In very small quantity, they gave a perfectly transparent globule of an amber or orange colour; in larger quantity, the vitrification was less perfect, and the globule more opaque. These characters perfectly agree with those of silex.

I have since had several opportunities of attending to the particulars of this case; and I have satisfied myself, beyond the possibility of doubt, that this siliceous sand is passed with the urine. On the first examination, I thought it possible that the sand might have been in the cup, and the urine have been voided upon it; but, upon attending to all the circumstances, it is impossible to admit of such a solution. In the first place, the red colour was owing to a thin coating of the alkaline lithates, ammonia, and lime. Thus, by treating them with

menstruum. Hence, no mistake could arise from the hasty exposure to the action of liq. potassæ, to which the sand was subjected. Nothing but the lithic acid coating was dissolved. This was satisfactorily proved by the addition of distilled vinegar, the separating and washing the precipitate, and re-dissolving it in nitric acid, which was attended with the usual appearances. On evaporation to dryness, the carmine red colour afforded sufficient proof of the presence of lithic acid, which was still further confirmed by the re-agency of the vapour of ammonia.

However, having treated a small quantity of the sand with nitric, muriatic acid, and caustic potass in succession, and washing after exposure to each re-agency, the residue being pulverised in an agate mortar, and reduced to a state of extreme comminution, was digested in potass and dissolved. The solution being supersaturated with muriatic acid, the silica was precipitated, and reduced by evaporation to a gelatinous mass. It was then evaporated to dryness, and was tried with warm distilled water by means of "Wollaston's Fountain of Compression," till the washings ceased to give a precipitate with nitrate of silver; the residue, being mixed with soda, fused into a transparent vitreous amber or orange-coloured globule. Quartz, treated in the same way, afforded precisely a similar result. The quantity of sand that could be obtained from all the specimens of urine was not sufficient to institute experiments upon a more extended scale without sacrificing the whole product, which I was unwilling to do, as I wished to preserve a small portion as a record of the most important feature in this interesting case.

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acetic or muriatic acid the lime was dissolved, leaving the lithic acid behind. On pouring off the acid, and adding oxalate of ammonia, oxalate of lime precipitated, and was recognised by its peculiar characters. The residual mass being treated with potass, the lithic acid was dissolved and taken up. Pouring off the solution, and supersaturating with vinegar, the lithic acid was precipitated in the form of a whitish powder.

The fact of these grains being covered with a coating of known urinary origin, fully proves that they must have been in contact with the urine sufficiently long to have had this coating deposited upon them. This could not have arisen from their accidental contact with the urine in the cup, because the urine was not acid, but neutral or rather alkalescent, as will appear when we consider the properties of this fluid. But further, they were not exposed to the action of the urine, after being voided sufficiently long to have acquired this coating. Therefore, it must have been deposited or secreted upon them in the kidney, or in some other part of the urinary organs. But in order to place the question beyond dispute, I washed the cup myself, and had the urine passed into it under circumstances which could not admit of deception, and I found the same sand in the urine voided as above-stated. Hence, there cannot be a doubt of their being passed with the urine from the bladder. I have had several specimens brought to me, and they all contain more or less of this siliceous matter.

A detail of the treatment in these cases would be inconsistent with the object of the present communication, which is the establishment of certain pathological facts, which have been doubted, and upon which the authority hitherto has been questionable. The morbid condition of the urine, however, is a necessary preliminary to the future remarks, and, indeed, essential to my purpose. I shall, therefore, proceed to this detail in each case.

In the first case, I received two specimens on the 27th of May, 1828;—the one passed after dinner the preceding day, was turbid from the mechanical suspension of a considerable quantity of muco-purulent matter, mixed with the alkaline lithates. On standing for a considerable time these subsided, leaving a transparent amber-coloured urine floating above.

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It reddened litmus paper, had a specific gravity of 1.026, and contained a considerable excess of urea.

The second specimen passed the following morning before breakfast, looked clear and almost like spring water; had scarcely any smell or taste; sp. gr. 1·02, faintly reddened litmus paper, and contained a very slight excess of urea. After standing, a greasy-looking crystalline film floated on the surface, which consisted, principally, of the triple phosphate, with a little muco-purulent matter. They were both serous.

I examined this woman's urine again on the 27th of August, 1829. It was turbid, opalescent, from the suspension of a large proportion of muco-purulent matter with the lithates of ammonia and lime, and which were of a whitish-yellow colour. There was also a small quantity of the fusible phosphates. It faintly reddened litmus, and the sp. gr. was 1·025. There was a great excess of urea, and it was serous. This unfortunate woman has very bad health, is very frequently attacked with severe illness, and has lately voided blood with the urine. She has not, so far as I know, passed any more gravel or calculi.

The second case, the general symptoms of which have been already stated, and, in many points, are analogous to those of the first, consulted me on the 22d of October, 1829.

The specimen of urine which she brought was turbid and opalescent. On being set aside, it became clear and transparent by the slow subsidence of purulent matter, mixed with the alkaline lithates, of a pale red colour. It was neutral or rather alkalescent, of a deep amber colour, serous, sp. gr. 1·020; no excess of urea; siliceous sand at the bottom of the cup.

28th. Siliceous deposit in very small quantity, but sufficient to distinguish it. The urine turbid from the muco-purulent matter, which, subsiding, the urine became clear, transparent, and of an orange colour. It was neutral, serous, sp. gr. 1.022; no excess of urea.

29th. Urine turbid as before, and exhibits the same general properties, reddens litmus, and contains rather more lithic acid; serous, sp. gr. 1.024; slight excess of urea: quantity of siliceous sand increased.

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30th. Urine turbid, opalescent, and wheyish; a small quantity of the siliceous sand at the bottom of the cup. On being left at rest, a quantity of whitish-looking powder subsided, leaving the supernatant urine clear, transparent, and nearly colourless. This urine was alkaline*, and the deposit proved to be the lithates of soda, ammonia, and lime, intermixed with a large proportion of fibreo-albuminous matter, such as I have seen frequently discharged from the kidneys, in diseases of these organs, attended with a serous condition of the urine. This urine contained albuminous matter. In other respects its properties nearly as those of the preceding specimens.

31st. She states she has not passed any appreciable quantity of the red sand since yesterday. The urine more plentiful, and she feels much relieved since passing the sand above described.

It would be useless to enter more at length upon the general history of these cases, as, with very little differences, perhaps of an accidental nature, what has been already stated comprises the principal circumstances, so far as they have come to my knowledge.

In reviewing the above facts, two or three interesting questions suggest themselves. Was this gravel the product of diseased action in the kidneys, and secreted as the oxalate of lime, cystic oxide, or any other morbid and unnatural product; or was it introduced into the stomach with the patient's drink? I shall consider the fact of this gravel coming from the bladder, as fully established; for none who will give me credit for veracity in the above detail, can for a moment question the renal origin of the sand, and its passage from the bladder with the urine. If it be a secretion by the kidneys, we naturally inquire, is it the result of morbid function without any supply of the materials,—as by the use of siliceous waters or other siliceous substances,—or does the morbid operation consist in the mere separation and aggregation of the siliceous matters in the water, or aliments ordinarily consumed by the patient? The spring waters in general use in this neighbourhood are hard, and contain lime and siliceous matters in solution; and

* She had been taking the liquor ammon. acet.

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indeed there is a strong tendency to urinary affections, partly in consequence of such a locality. This patient, however, is a native of Gloucestershire, and has been resident here only about six weeks; and during this time has daily used the common pump water of the place. She also states that her grandmother obliged her, when young, to drink a great deal of sea-water, and she thought the sand thus introduced might now be appearing; but as many years have since elapsed, we can hardly look to so remote an origin. There is one important circumstance in her history which I must not omit, that urinary disorders prevail in her family.

It occurred to me that there might be a possibility of a quantity of this very fine siliceous sand being pumped up with the water, and remaining in mechanical suspension, as I have occasionally seen. In this way, perhaps, some might be disposed to account for its appearance in the urine; but the objections to such a solution are many and various. In the first place, others should be equally liable under similar circumstances; yet, notwithstanding I have been in the habit of attending closely to the properties of the urine, I have never met with any instance of a similar nature, and it is highly probable that I should have met with them had they occurred. Secondly, I have repeatedly been furnished with portions of the water, pumped up in haste, and at a time too when she was using it, and passing this siliceous matter, but I could never detect the smallest portion of silex in *mechanical suspension.*

But even granting that it might, and actually had been so introduced in this instance, such a solution involves still greater difficulties. If introduced into the stomach, does the silex undergo digestion, become dissolved, and thus enter the circulating mass, and so arrive at the kidneys? If so, it is evident that the kidneys must reorganise the silex to precipitate it in the crystallized form, for this can hardly be effected any where else *. It may be asked, could not the silex be

* But there is a still more weighty objection. If the seeds of pears, apples, and fruit-stones, &c., can resist the solvent powers of the gastric juice, we cannot well conceive them adequate to the solution of so refractory a substance as silex, at least under ordinary circumstances.

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carried whole, without undergoing digestion, to the kidneys, and ultimately make its exit with the urine? There are, however, a number of objections to such a theory. In the first place, fruit-stones, grape-seeds, &c., which resist the digestive powers, are always voided with the excrement, and have never been observed to pass with the urine. Secondly, before reaching the kidneys * they must pass with the chyle into the circulating mass, and hence go the round of the circulation. Now, we cannot conceive such hard gritty irritating matter pervading the more delicate tissues and finer vessels, without exciting violent inflammation and speedy disorganization.

Upon reflection, and after close attention to the circumstances, I am disposed to attribute the appearance of this matter to a morbid condition of the kidneys themselves, either generating the substance itself by a morbid process, or else separating it from the drink or aliment, and aggregating it into the masses and forms under which it appears mixed with the urine.

In support of this view, I would observe that there is manifest disease in the kidneys as well as bladder, and a morbid condition of the uterus. There is evident tendency to urinary affections, attended, as is mostly the case under such circumstances, with an impaired state of the general health. There is a great analogy between the two cases, and between the general symptoms in each.

There is, however, one fact clearly established by this paper, namely, that siliceous sand may—however introduced, or whencesoever its origin—form, in some cases, a portion of gravel, and from which we may infer that it may sometimes form a portion of urinary calculi.

* There can be no doubt that fluids sometimes reach the kidneys and bladder by a less circuitous route than the round of the circulation. The means by which this is effected, are at present involved in so much obscurity, that nothing decisive has been determined relative to this question. It has been supposed to be accomplished by transpiration, and if it has so, it is evident that silex in the solid form, though in a state of ever so minute mechanical division, could not be thus conveyed to the kidneys or bladder.

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On the Word Bedolah, *or* Bedolach, בדלח. *By* Dr. J. J. Schmidt, *of St. Petersburgh.*

EVERY attentive reader of the Mosaic history of the creation must have felt some doubts respecting the true meaning of the word *Bdellium*, as it occurs in the second chapter of Genesis. Of the land of Havilah, which the river Pison "compasseth," it is said that "the gold of that land is good; there is bdellium and the onyx-stone." It is scarcely possible to believe that the bdellium here mentioned signifies nothing more precious than the gum now known by that name. In the eleventh chapter of Numbers it is said that "the manna was as coriander seed, and the colour thereof as the colour of bdellium." Here the gum might naturally be regarded as the object of comparison; yet many commentators, both ancient and modern, have supposed it to be the pearl, because manna is white, and Havilah is understood to have been situated on the Persian Gulf, which abounds in pearls. We certainly may, in this way, give an apparently suitable explanation not only of the valuable productions described as existing in the country through which one of the rivers of Paradise flowed; but more particularly of those objects with which manna, in regard to its colour, is compared. There yet remains, however, this very important objection to be got rid of. Why should pearls, if they be meant in the two passages cited, be there called Bedolach, and not דר, Dare, as in every other part of the Old Testament? It is evident that the interpretation "pearl" has only gained acceptance because that term, in the passages where Bedolach occurs, would produce an agreement in the sense; and all further philological confirmation is, therefore, supposed unnecessary.

In my opinion, the Bedola, or Bedolach, of the book of Genesis, is neither pearl, nor the gum called bdellium, and that it must denote some other precious thing,—perhaps, lapis lazuli. I am aware it may be asked, whether it can be admitted that Bedolach belongs to the mineral kingdom, since it is not expressly called a stone, as is the case, for example, with the stone shoham? But this objection is of little importance; for,

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were everything clearly and distinctly designated there would be at once an end of all obscurity and conjecture! How common it is, in speaking of precious stones, to call them by their own particular names!

Lapis lazuli is often made to perform an important part in the Indian cosmogony and mythology, and in those of Thibet and Mongul, which are borrowed from the Indian. Its Sanscrit, and also its Thibet name is *Veidurya*, or *Veiduryah*, which probably gave rise to its Mongul name—Bedurya. In this appellation of lapis lazuli the Mosaic Bedolach may be found without the least difficulty; for languages in general, and more particularly the Asiatic, afford so many examples of the conversion of R into L, and of V into B, or rather of the confounding of those letters, that no linguist can hesitate to admit the fact. Moreover, I am not ignorant that the sapphire of the ancients is pretty generally supposed to be lapis lazuli. This opinion, however, has no foundation in authority; but I am willing to leave to it all the credit to which it may be entitled.

Of the valuable productions of the mineral kingdom mentioned in the Buddah books, the following four may be regarded as the principal: namely, Gold, Silver, Veiduryah, and Padmaraga (ruby). The Ugyu, or Gyu (the oriental Zode), Marakata, or Marakta (Emerald), Crystal, and others, occur less often. According to those books, also, the eastern declivity of Sumeru, the fabulous mountain of the world and seat of the gods (by which some understand the high land of Thibet and Great Tartary), consists of silver; the southern, of veiduryah; the western of padmaraga, or ruby; and the northern declivity, of gold. In these books crystal is sometimes put for ruby, and, according to them, the principal eastern river, or the Ganga (Ganges), flows on silver sand; the southern, or the Sindhu (Indus), on veidurya sand; the western, or the Backtschu (perhaps, from *baktra* and *chu*, a Thibet word signifying "water"), on crystal sand; and the northern, or Sisita, on gold sand. It is said of these four streams that they spring at a short distance from each other,—from the sides, indeed, of the square Lake Map'am, in the centre of which grows the tree Jambu (rose-apple: it takes its name from that part of

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the world called Jambudwip), whose falling fruit serves as food for the serpent (*naga*), who inhabits the lake. We observe here, as in Genesis, four rivers take their source from a reservoir, only that in Genesis the direction of the course of each of them is not described.

I therefore conclude, that Bedola, or Bedolach, has, perhaps, a twofold meaning: that, in Genesis, chap. ii., it most probably signifies lapis lazuli; and that, in Numbers, chap. xi., it may mean the well-known gum with which manna is supposed to be compared.

On a Prismatic Structure in Sandstone induced by artificial Heat; and on certain Prismatic Rocks found in Nature, including the Columnar Sandstone of Dunbar. By J. Mac Culloch.

The connexion which may be inferred, between the artificial change in sandstone, that forms the subject of one part of this paper, and that natural arrangement of the same kind which occurs in certain rocks, has induced me to describe the whole in one memoir. I will not pretend to pronounce on the degree of probability that exists respecting this presumed connexion; but it is certainly such as to warrant a further repetition of similar experiments, and a further examination of analogous phenomena occurring in nature. However unfounded that supposed connexion may be proved, the results of the artificial process are intimately connected with so many analogous facts occurring in other solid substances, that it must be considered as only one example of a most important general law, the effects of which have scarcely yet been examined, and which opens a wide field of experiment to those whom leisure and opportunity may tempt to investigate it. If, by such experiments, that law may be ultimately shewn to have an extensive operation, such is its nature, that its action cannot be confined to the narrow extent of our furnaces and crucibles. That which regulates the affinities of the substances in these limited operations, has regulated all the chemistry of nature; and the globe itself is only ruled by those powers which determine the combinations of the least of its atoms.

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Dismissing this part of the subject for the present, the artificial results which form the first part of this paper are interesting, even in an abstract chemical view, from their connexion with the analogous changes which so many other solid bodies undergo from the action of heat. In any point of view, a fact so solitary and unexpected is deserving of record.

With respect to the geological facts which form the second division of this paper, they are, indeed, too rare to be very satisfactory, since they seem rather to belong to the class of exceptions than rules. Yet I trust it will appear, that they are united by one common band which removes them from the class of insulated facts, and gives them a more general interest. Increased observations will probably add to the number; and by tracing various circumstances not to be found in these, by which they may be accompanied, geologists may ultimately establish a principle capable of explaining phenomena which have hitherto been a source of no small difficulty. Should even the phenomena here described prove incapable of being connected with any important general principle, their singularity and rarity render them worthy of record. The appearances at Dunbar, in particular, have been so much misunderstood, or so inadequately examined, that it seemed necessary to place them in a clearer point of view. I can only regret the want of materials sufficient to furnish a better connected train of evidence, on a subject which appears to comprise important consequences in geological science.

Some phenomena, which take place in glass, and in other solid bodies exposed to a continued heat incapable of bringing them into a fluid state, induced me long ago to undertake a series of experiments on this subject. Some valuable results were obtained, which I shall probably take some future opportunity of communicating through the medium of this Journal; but the impossibility of preserving a continued heat for a sufficient length of time in a common laboratory, ultimately caused these experiments to be abandoned. Among other substances, it was attempted to change the internal arrangement of those rocks, which have been supposed to owe their origin to igneous fusion, by the application of a heat incapable of producing that ultimate effect. But the defects of the furnaces already alluded

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to, rendered it necessary to abandon this train of investigation before any satisfactory or decided results were obtained. Thus the subject slept, without being forgotten, till it was revived by the circumstance which I shall now proceed to communicate.

In taking down a blast furnace at the Old Park Iron-works, near Shiffnall, which had been sixteen or eighteen years in constant work, the hearthstone was found to be broken, and, at the same time, to present a remarkable reticulated surface. On examining the fragments after removal, it was found that they were split into polygonal compartments, and some of them were therefore preserved. One of these was subjected to my examination by the Duke of Northumberland; in whose possession it now is, for the satisfaction of those who may wish to refer to the fact and its authorities.

The hearthstone, it is well known, lies at the bottom of the blast furnace, so as always to be covered with melted iron at a high heat, and to this heat it was unintermittingly exposed during the whole period of years above-mentioned. The kind of stone usually selected for this purpose is a fine-grained white sandstone, containing a very small proportion of argillaceous earth. It is either found, or imagined, to be the least liable to fuse or crack in this high degree of heat. I have not been able to procure natural specimens of the rock from which the hearth in question was made; and it is now impossible to ascertain its exact nature. It can only be concluded that, according to general practice, the hearth in question was made of this variety of sandstone.

The thickness of the fragment which I examined was about ten inches, which is understood to be that of the hearth. It is everywhere divided into prisms, which, in some places, reach through the whole thickness, but, in others, only penetrate to a certain depth; being gradually blended beneath with the solid mass. These prisms are sometimes tolerably regular; at others, their sides undulate, but so as still to fit each other, without intervals, wherever they approximate. Their diameter varies from one inch to the half of that, and they contain, generally, five, six, or seven sides. They are not absolutely in contact above, but they gradually approximate below; appear-

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ing to have been most separated where the heat has acted most immediately; though from this appearance it is not unlikely that this separation has been produced by mechanical causes. Their structure being tender, it is easy to imagine that the incessant motion of so heavy a fluid as iron along the surface of the hearth, had worn the sides of the prism so as gradually to permit it to insinuate itself among them, and thus to cause a separation towards the upper extremities. It is essential to attend to this circumstance, becauses it involves an important question respecting the columnar trap rocks. In the desiccation of clays containing much water, a division of the mass takes place, which causes it to separate into parts of an irregular prismatic configuration. This is the effect of drying; and, in consequence of the diminution of bulk thence resulting, the prisms are separated from each other by considerable intervals. In the columnar trap rocks, on the contrary, the prisms remain in perfect contact; proving that the configuration does not, in this case, depend on a general diminution of the bulk of the original mass. In the case of the sandstone in question, it is certain that it can undergo no loss of water sufficient to produce a separation of the mass from that cause; while the absolute contact of the prisms, where they have not been exposed to causes of waste, proves that the separation into these forms is the consequence of circumstances of some other nature.

On examining with a magnifying glass the texture of this sandstone in its present state, the most remarkable change is the want of angularity in the particles of the sand. It is true that this may have been the original character of the stone, but it is accompanied by an occasional vitreous splendour in some of them, which seems to prove that their surfaces have been slightly fused. It does not follow, however, that this is a fusion of quartz; it is more likely to be a glass produced by a combination of the argillaceous earth, where the two have been in contact. The small opaque particles which seem to be intermixed with the grains of quartz, may possibly consist of the supposed argillaceous earth of the original stone; but they are so difficult to determine, that I am even inclined to doubt if this specimen ever contained any clay, and whether it is not a simple sandstone. It has certainly undergone some change;

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but that is of such a nature, that the lens, at least, is incapable of determining what it really is. It is necessary yet to observe, respecting the character of this burnt sandstone, that instead of being white, as in its original state, it is of a grey colour; and, on examining it with the lens, it is easy to see that particles of dark grey and transparent quartz are intermixed with the colourless and transparent; so as to produce this effect. The nature of this change in the quartz is not intelligible; and the specimen presents no other appearances that I could discover, capable of throwing any light on the subject.

It had formerly been proved, by the occurrences so common in glass-houses and iron-furnaces, among the glasses and slags which have remained long in the fire, that, in a long continued heat, or rather during a process of slow cooling, these substances were capable of crystallizing, so as, on solidifying, to lose their ordinary character of glass, and to assume the crystalline arrangement found in many kinds of rock. The experiments of Mr. Watt and others, on stones fused and treated in the same manner, confirmed their accidental results. But it remained to be proved that heat applied to rocks in a degree inferior to that which is required to effect their fusion, was capable of changing in an analogous manner the internal arrangement of their particles, as happens in the case of some other solid bodies. This was the object of the experiments which, as already mentioned, I undertook without success, but which this accidental experiment has proved to be possible.

But the change, in this case, is not a change of that which is properly to be considered the crystalline arrangement of particles. There appears to be no such arrangement in sandstones; nor are there any of the characters of true crystallization in the prismatic structure which has followed the action of the heat in this instance.

What the real nature of this analogy between the crystalline and concretionary structure in rocks may be, it is impossible to conjecture, ignorant as we are of the real nature of both these processes. In crystallization, we can see that the crystals, assuming the integral form or molecule, is the most simple, consists of similarly formed parts, divisible, as far as we have

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yet discovered, without end. In the concretionary structure, no such divisibility occurs; but the pseudo-crystalline form, which it assumes, is only divisible into shapeless parts, which bear no relation to each other, or to the general figure of which they are the constituents. Yet it is probable, that even the crystal must be constituted in the same manner; and that the ultimate particles, or atoms of matter of which it is composed, have no forms coincident with that of the molecule which they generate.

Whatever the differences between true crystallization and the concretionary structure in rocks may be, there is, at any rate, an analogy sufficient to show that both arrangements have been produced by certain attractions among small particles, tending to arrange them in particular forms. In the crystalline structure, that attraction has been exerted, either among simple atoms, or among definite combinations of these, free to act and move in the directions to which they are impelled by certain unknown laws, in consequence of the fluidity of the mass of which they formed the parts. In the concretionary arrangement, or the contrary, from every thing as yet known, the motions of the parts have been limited; either by their figure, bulk, or imperfect capability of motion; or in consequence of previous determined combinations, perhaps of a more complicated nature, and of the want of a fluidity, enabling them to assume the disposition to which they might possibly have tended under more favourable circumstances.

It is impossible, at present, to illustrate this obscure subject further; but it is evident, that on examining the various experiments that bear on this question, the action of fire is capable of producing both the crystalline and the concretionary arrangement.

In simple crystallizable bodies, heat changes the crystalline arrangement, even though the substance remains solid: in the case of this sandstone, it has generated the concretionary structure. In glass, it seems to produce a crystalline arrangement also; but, in this case, it is probable the change does not resemble that which takes place in metals, but that the nature of the combination of the integrant substances is altered. In fluid rocks, the parts being free to move, and the particles

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of the different substances to enter into new combinations, it generates the crystalline structure; and here, also, we may observe that there is an analogy between the effects, the same cause producing the crystalline structure where the rock is fluid, and the concretionary where it does not lose its solidity.

If, therefore, the limited experiments which have been hitherto made in our laboratories have thrown light on the crystalline structure of certain rocks, by proving that this may have taken place from a state of igneous fluidity, the accidental experiment here described may, perhaps, tend to throw a similar light on the concretionary structure of rocks. The experiment, indeed, is as yet solitary; but even a solitary experiment is the result of a law, and that law must act in every other case, where the circumstances are the same. We may therefore infer, without much hazard of error, that the long-continued application of a certain degree of heat to a sandstone of certain composition, will, in all cases, under identical circumstances, generate in it the prismatic concretionary structure. It will hereafter be shown, in the second part of this paper, how this principle may be applied to the solution of some difficult cases in geology.

How far we may venture to extend this principle from sandstone to other rocks, is a question to which no decided answer can be given. It may easily be conjectured that I here allude to the trap rocks, which present examples of the concretionary structure the most remarkable and frequent among those with which we are acquainted.

To illustrate this subject as it merits, it would be necessary to put it to the test of direct experiments modelled on that which is here recorded. What time would be necessary to produce a result, if a result is to be produced, is uncertain. I have attempted in vain, by inquiries among iron-founders, to learn what length of time the exposure to heat was required to produce the prismatic configuration of the hearth sandstone. But the destruction of a furnace is a rare occurrence, and the time which it remains at work is such, that there seems no possibility of discovering the *minimum* of time required for this effect. Still, the experiment might be tried in the hands of those who are conveniently situated for that purpose, by

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exposing various trap rocks, as, for example, basalts, of sufficient bulk, to a heat short of that required to fuse them, and for a sufficient length of time, to be determined only by repeated examinations. Should such a result take place as that which has occurred in the sandstones, we should then be in possession of a valuable fact from which to deduce a train of geological reasoning respecting the figured traps, which must now be supplied by conjectures and analogies. What may be offered on that subject I shall now defer, till the natural appearances, which resemble the artificial one already described, have been detailed.

I shall now, therefore, proceed to describe some appearances in nature, which appear not only to illustrate the preceding experiment, if such it may be called, but to show the connexion which it has with similar phenomena in the structure of rocks. That connexion it must, however, be admitted, hangs on a hypothetical view of a cause; or rather, on an inference regarding the probable action, in this particular instance, of a certain state of things which is now almost universally admitted to have existed. But the nature of the reasoning on this subject will be better understood when the appearances themselves have been described.

That substance known by the name of "columnar ironstone" is probably familiar to most mineralogists. It is found under many different modifications, and these principally relate to the sizes of the prisms of which it is composed. For the objects of the question in view, although it is not useless to inquire respecting the causes of this peculiar configuration, it is impossible to reason very satisfactorily from it, unless the geological situation of all such specimens were carefully examined. Unfortunately, the only instances with which I am practically acquainted are those which occur in Arran, which I have described in my work on the Western Islands of Scotland.

These examples are noted for the large size of the prisms, and for the facility with which they separate into short joints; while, in one of them, there is further a peculiar character produced by a groove or channel on the ends of the prisms, and parallel to their sides. These peculiarities, however, seem

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to have no particular connexion with the question now under consideration.

It is proper to remark, however, that this bed of ironstone, as it is called, might equally be termed a shale, in which the red oxide of iron abounds in an unusual degree. Such a composition of clay and iron is, indeed, that of all iron stones. In this place many of the shales are highly ferriferous, being of red, yellow, and purple colours; so that while the exact limits of ferriferous shale and argillaceous ironstone cannot be defined in the abstract, neither, in this place, can it be said that many of these beds are not formed of the latter substance. The shales also pass so gradually, in respect to composition, into the columnar ironstone which occurs near them, that the differences are often not perceptible. The columnar bed under review might, indeed, with equal propriety, be called a ferriferous shale; or else the shales of the same character should rank, like it, with the ironstones.

I must remark that the surface of the columnar ironstone in this place being exposed, there is ready access, not only to this bed itself, but to all the surrounding rocks. These rocks are sandstones, shales, and limestones, of various characters; all of them appertaining to that series of red sandstone which forms this quarter of Arran, and which extends across the whole of Scotland to the eastern shore. Besides these, veins of trap occur in the immediate neighbourhood; some of them intersecting the strata, and others following the same parallel course, so as to appear interstratified with them. It ought still to be observed, that although masses of superincumbent trap are not found at this particular place, they abound in other places in Arran, not far off; forming, indeed, a considerable portion of this island. As the reasoning which it is intended to apply to this phenomenon applies equally to the case next to be described, I shall not now enter on it, but defer the whole argument till the history of this also has been given.

In the island of Rum there is a mass of sandstone, similar to that of Sky, which, in some places, is covered by trap rocks of various characters. Under the hill of Scuirmore, this sandstone lies under that basaltic rock so remarkable for the quantity of heliotrope which it contains. In one place the bed is

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divided naturally into regular prisms, of a diameter not exceeding two or three inches; which, being detached, may often be found on the beach. It is not possible to ascertain the extent of this appearance, because the greater part of the cliff is absolutely inaccessible. Standing alone, it is not a circumstance, perhaps, to have excited much interest; but when connected with the phenomena next to be described, it forms a valuable addition to a collection of facts as yet, it is true, rather too limited to be of a very satisfactory nature.

It has long been known, that a collection of columns of considerable size exists at the pier of Dunbar; and it has been examined, perhaps, by every mineralogist who has resided in or visited this country. This rock has been known by different names, having been called a red basalt, and a columnar jasper. It will presently be seen that it bears no relation to basalt, being a stratified rock; and that, instead of consisting of one substance only, the prisms are formed of different minerals or rocks, appointed by the common bond of juxtaposition and form. The probable causes of their variety, and of their real nature and origin, will be rendered sufficiently apparent when their true geological connexions are described.

The whole of this shore consists of that red sandstone already mentioned as reaching from Arran across the centre of Scotland. It is true that, between the two, there is no continuity of connexion visible; but their relations to the subjacent and superincumbent strata, and their characters, are, in all essential points, the same. This sandstone is the old red sandstone of certain geologists.

The substances of which the series consists at this place are, principally, a red argillaceous sandstone, with which are interstratified red ferriferous shales, common lead-blue shales, white sandstones, and some slender laminæ of an impure calcareous rock. The principal limestones lie higher up in the series, which, as is well known, is followed by the coal-field of this coast.

Another circumstance respecting the general structure of this country must yet be noticed, to render the geological description complete. It contains, in several places, insulated and superincumbent masses of trap, together with veins of the

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same rock. The Bass, Traprain Law, and North Berwick Law, are the most conspicuous of these: but many of smaller dimensions, scarcely raised above the general surface, occur in other places. It is further obvious, that many of these detached masses of trap are undergoing a rapid decomposition; gradually mouldering into soil, and leaving exposed, or at least only covered with loose clay and earth, the stratified rocks which they once concealed.

I have yet to remark, that the general inclinations of the sandstone strata are various, but almost always inconsiderable; and this comprises all that appears necessary to render the geological structure of this district intelligible.

The strata of the red sandstone, as they appear near Dunbar, dip to the south-east, or thereabouts, at an angle of nine or ten degrees; and, in most places along this shore, they present a very consistent regularity. Immediately at the town, however, they are confused and irregular for a small space; and here also there are intermixed with the red strata some beds of white calcareous sandstones veined with red.

In carefully tracing these beds to the pier, where the columnar appearances already mentioned are found, it is easy to see that this mass of prisms is a portion of one of these beds, or that the bed is, in this particular place, divided by a prismatic configuration. The area of the columnar part is such as might be contained in a space of 80 or 100 yards by 30; but accuracy in this point is of no consequence: neither is it possible to be obtained, as there is such a gradation from the simple undivided stratum into the prismatic part, that the limits of the latter cannot be fixed.

It is, indeed, easy to see, on a careful inspection, that the prismatic configuration is assumed by the common sandstone bed in a very gradual manner. The mere indications of an irregular vertical division are first seen; and these are followed by a definite outline on the surface, making the figure of a prism which is still incapable of being separated from the surrounding parts. By degrees, these markings increase, so as to leave hollowed lines in the rocks; and, in a further progress to the perfect structure, the prisms are not only seen to be distinct, but, having been irregularly broken off at unequal alti-

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tudes, they form an uneven columnar floor resembling the well-known causaway of Staffa. This very perfect part of the structure does not, however, occupy a space of more than a few square yards.

As the columnar division takes place in the manner most common in the trap rocks, namely, at right angles to the stratum, the vertical inclination of the protruding columns is ten degrees, or thereabout. Their diameters vary from eighteen inches to two feet, or more; and, as far as can be discovered, their greatest altitude is about ten or twelve feet. It is not certain, however, that this is their total height, or the greatest thickness of the prismatic part of the sandstone bed; as a great part of the surface has been broken or worn away during the lapse of time. The angles of the prisms vary in number;

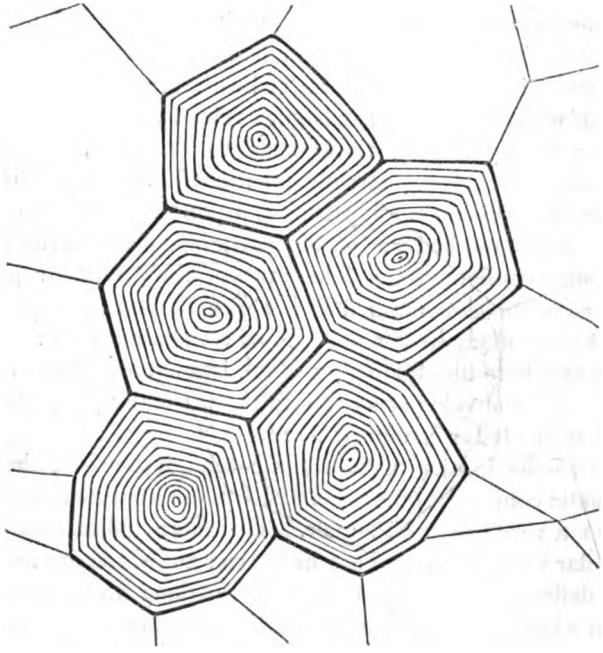

Fig. 1. *Concentric Structure of the Columns at Dunbar*

but, like the cases most frequent in the trap rocks, forms of five, six, and seven sides are the most numerous. They do not

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appear to be anywhere jointed, but they break at right angles to their axes. Neither do they present any marks of that peculiar channelling on the surface which occurs in the shale of Arran.

Independently of this decided configuration, there are some indications of an internal structure in this sandstone bed, connected with the prismatic form, which it is necessary to notice.

Where the first indication of the future division into columns takes place (see *fig.* 1.), the primary or bounding lines by which they are indicated are often filled with a series of concentric lines, distinguished by some differences both of colour and texture, and commonly by white lines on a red ground. This circumstance presents an interesting analogy to the case

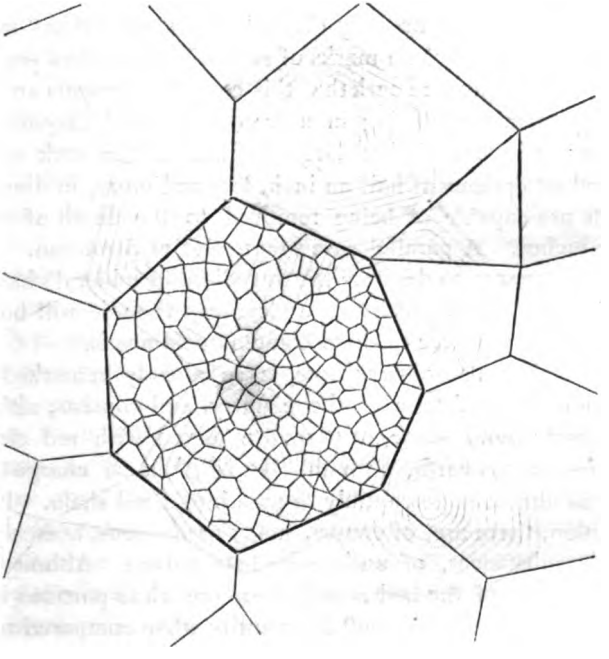

Fig. 2. *Subdivided Prismatic Structure of the Columns at Dunbar.*

of the basalts, in which, from their eventual decomposition in successive crusts, it appears probable that they possess an

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original internal corresponding structure, although that is not visible in the fresh rock, perhaps in consequence of the nature and uniformity of the colour. Having, in another communication, stated the facts and arguments on both sides respecting this process in basalts, I need not notice it further here. But it is plain in the case of this sandstone, that any exfoliation which the prisms may eventually undergo will not be the consequence of superficial decomposition from the action of the air, but will have its foundation in a peculiar concretionary structure of a laminar nature concentric to the axes or circumferences of the prisms.

Another remarkable variety of internal concretionary structure also occurs here (see *fig.* 2). In this a minute columnar tendency within the principal column is indicated in the same manner by variations of colour. Whether this is ultimately to become actually divided into small prisms cannot be conjectured; but no marks of such division are as yet to be seen. But I must remark that this case also presents an exact analogy to one occurring in a trap rock (hard claystone) in Morven, where a similar large division of the rock is subdivided into prisms of half an inch, less and more, in diameter, which are capable of being removed to the depth of two or three inches. A parallel case occurs also at Strontian. It is, lastly, necessary to describe the variations of mineral character which this bed of sandstone undergoes, when it will be seen what the true nature of these disputed columns is.

The bed, in its ordinary state, is, as already remarked, the common red sandstone of the country, and consists, either of red quartz sand alone, or of quartz mixed with red clay, or with calcareous earth, or both. It is often so charged with clay, as almost imperceptibly to pass into a red shale. In the transition, there are, of course, many arenaceous, scarcely definable substances, of an intermediate nature. Although the general mass of the bed is red, there are white portions intermixed; but these are small in quantity when compared to the red part of the rock.

It is very perceptible that the greater part of this bed, where it is here accessible, is harder than this rock generally is found to be in this neighbourhood. But it becomes decidedly more

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indurated where it approximates to the columnar parts. These are, in many places, extremely hard; but that character is not universal, and the causes of these differences are very easily explained, when the variations of character in the unaltered part of the bed are considered. Some of the columns thus consist of sandstone highly indurated, while others are formed of the same indurated shale as that of Arran already described, and might thus with the same propriety be called columnar ironstone. In some places the columns consist of jasper; that is to say, the argillaceous sandstone is here indurated to such a degree, as to put on the appearance and characters of that rock. It is thus plain, that the term columnar jasper has been improperly applied to this whole mass of prisms, as the occurrence of this particular substance is but partial. It is indeed so irregular as not even to exist necessarily throughout the whole of any one column, or a single column is not always formed of one substance; as for example, of jasper, of ferruginous hard shale, or ironstone, or of indurated sandstone. On the contrary, the jasper often forms a very small part of one column, and all these substances are sometimes found intermixed in the same. This is a circumstance, therefore, which may easily be understood from considering, as already hinted, the mixed and irregular nature of the sandstone bed where it retains its natural form and disposition.

It is now necessary to inquire respecting the probable cause to which these prismatic concretions owe their forms and origin. It is no assumption, if a fact, to use this language. If there is anything certain in the whole range of geological science, it is that the secondary strata have been deposited from water, at least essentially, in their present predominant forms; and from this cause they assume that simple stratified appearance which is the natural result of this process, and on which it is quite unnecessary to dwell. But, in many places, they are partially modified, or present appearances of a limited nature, inconsistent with their prevailing characters, and probably, therefore, the consequences of actions of a different and local nature, simultaneous with, or posterior to their deposition.

The structure in question must be ranked with these, as no depositions of earths from water, under the usual circum-

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stances, can be conceived capable of producing the effects under examination. Their partial and limited nature also proves that two causes have acted in this case.

The reader who has attended to the statement at the commencement of this paper, is doubtless prepared to expect that the prismatic structure in the instances which have now been described are attributed to the action of heat. I shall attempt to arrange the arguments which I have to offer in support of this opinion in the most intelligible form. I make no scruple in assuming that which all rational geologists now admit, namely, that the trap-rocks are the produce of igneous fusion; and, in taking this for granted, the source of heat, so far as it concerns these sandstones, is established.

Now, in very many cases where trap rocks are found in contact with certain strata, these are found to have undergone particular changes within certain limited distances of the places of contact. Thus, shale is converted into siliceous schist, earthy limestone is crystallized, highly argillaceous limestone becomes chert, and various argillaceous rocks pass into varieties of jasper.

The relations between the several varieties of this latter substance thus produced, and the various natural or original strata from which they have been formed, are those which particularly bear on the present question. The purer clays are found to be converted into fine jaspers of a resinous aspect, as I have shewn in a former paper in this Journal. The argillaceous white sandstones are changed into jaspers of a less perfect character, which vary also in the same place, according to previous varieties in the composition of the different beds from which they have been derived, and to which they may still be traced. The red argillaceous sandstones are converted into red jaspers, of characters equally varying, according to the previous nature of the original rock, or to its distance from the source of the heat; and the examples of this are so familiar to all practical geologists, as not to require being specified. The shales which contain much siliceous matter, are, in the same situations, converted into ironstone and into jasper; and, in the same manner, the incompact highly ferruginous clays become, according to their capacity

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and other circumstances, one or other of these substances. That the effects there enumerated take place in innumerable instances is fully demonstrated. That they sometimes do not occur where they might be expected is equally admitted. But it is not necessary to show reasons for these exceptions, as the discussion would be here out of place, and has often been before the public in many different forms.

It is therefore proved by natural appearances, or in the great experiments of nature, that the action of heat existing in the trap rocks is capable, among other changes, of converting red argillaceous sandstone, and ferrugino-siliceous shales into jasper, and the latter, under other modifications, into ironstone. It was also shewn in the first part of this paper, that the action of artificial heat was capable of producing the prismatic structure in sandstone.

It remains to produce an instance in nature where the presence of trap and the prismatic form in the sandstone near it concur; and this example is found in the instance in Rum above mentioned. It will probably be found in many other places, when the facts and reasonings which this paper now promulgates for the first time shall become known to geologists.

As the action of heat, therefore, or the presence of trap, and its presumed influence over the strata, have been shewn capable of producing, separately, either the prismatic configuration, or the change to jasper in certain modifications of sandstone, and that instances of each of these have been adduced, it does not seem at all illogical to conclude, that the same cause may produce both these effects united. I must regret that I cannot produce an actual instance of the contact of trap with this union of the jaspideous character and the prismatic structure, but future researches will probably discover them.

In defect of this, we can only be guided by reasonings from analogy, in attempting to explain the causes of the peculiar nature of the three examples described in the second part of this paper.

The case of Rum is indeed proved; since, as already observed, the trap is found in contact with the prismatic sandstone. In Arran, the presence of trap, in various parts of the tract in which the prismatic ironstone occurs, is evident; and

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from the facility with which this rock is often decomposed and removed, and the great destruction of substances here visible, it is not improbable that some mass of it has once been in contact with the columnar part of the bed of shale which forms the ironstone under review.

In describing the country about Dunbar, I shewed that detached superincumbent masses of trap occurred in various places, and that, in many, they gave striking marks of rapid decomposition. The ultimate removal of such masses is proved by their gradual evanescence in other parts of Scotland; in many districts of which they may be traced gradually diminishing in size as they recede from some larger mass, till they vanish, leaving proofs of their former existence in this peculiar nature of a soil, which those who have once been intimate with it will never fail to recognize. The conclusion to be drawn from this reasoning is, that one of these masses of trap has originally covered the prismatic sandstone of Dunbar, and that to the influence of this is owing the change both of structure and mineral character which it has undergone.

In terminating these remarks, I do not feel it necessary to account for exceptions; for it may be said, that this effect, if depending on that cause, ought to occur more frequently. The common answer to this often-repeated objection against all general conclusions from particular facts is sufficient. Nothing can be pronounced an exception till all the concomitant circumstances, or those capable of modifying the presumed results, are known.

Of these we know little at any time; and the objection, such as it is, applies equally to every case where the influence of trap on the neighbouring rocks is visible.

It now remains to inquire how far the preceding facts and reasonings can be applied to the often-discussed question of the prismatic configuration of the trap rocks. Here we have, at least, some strong analogies to guide us. It is certain that, after such fluid masses had become solid, they must, for a long period, have retained a considerable degree of heat, and undergone a process of slow cooling. It has, indeed, been supposed by many, that the prismatic configuration in the traps has taken place during the act of consolidation, or that it was a

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crystalline process. To apply that term to it is incorrect; it is a concretionary structure, and, from the examples in sandstone which have been described, it is more likely to have taken place in the solid than in the fluid mass. The same reasoning applies to the prismatic lavas, which, indeed, are themselves sufficient to prove that this structure is the result of heat, at whatsoever period and state of the lava it may have acted.

Finally, as in the Dunbar sandstone the concentric concretionary structure of the prisms has been shown to exist, and that this arrangement is common in the columnar traps, as is proved by their decomposition, it is probable that in the latter, as in the former case, it has been produced during the solid state of the rock.

It is impossible to illustrate this subject further, in the present state of our knowledge; nor is it very likely that any satisfactory proofs of it can be derived from nature. It is not easy, at least, to conjecture how such proofs can be discovered. That it might be put to the test of direct experiment I have already suggested; and repeating the hope that such experiments will be instituted, I shall now conclude this communication.

Experiments on Indigo.

[From a Calcutta Journal.]

Chemists in Europe, who have engaged in the examination of indigo, have generally had to deal with the prepared colouring matter as manufactured for the market; and have therefore limited themselves to the separation and measurement of the foreign ingredients with which it was contaminated,—to the properties of the pure colouring matter itself,—and to the analysis of its chemical composition. The rationale of what passes in the process of the manufacture may be, and has been, deduced with tolerable certainty from the discoveries thus made; but where we have the whole fermentation carried on among us on an immense scale,—when we have the indigo in its nascent

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state, and in its colourless soluble state too, capable of being submitted to tests and processes, it becomes a matter of curious interest to follow the changes of this singular substance, and compare them with the theories formed in the laboratories at home.

It is, however, more than a mere matter of curiosity to set on a sound basis the causes of the different manipulations, and to examine the real effect of processes in which different manufacturers have a diversity of practice;—although it must be confessed that, setting aside the difference of quality in the plant, from season, care in its cultivation, soil, and other causes, the business of the vats is so simple as to allow of little deviation in practice or result.

The most convenient manner of bringing before the reader the various subjects of a short series of experiments which I made this season, in conjunction with an intelligent friend engaged in an indigo establishment, will be to incorporate them with a relation of the general process of manufacture; but I think it will save a good deal of repetition and explanation, to give in the first place an epitome of what has already been written on the subject; that is, on the qualities and properties of indigo itself, which have been elaborately examined by Bergman, Berthollet, Chevreul, Thompson, Crum, and latterly by Berzelius. Their notices extend to minute details of the action of every neutral salt, acid, and alkali of the chemical calendar; but the general results which are likely to be useful to the manufacturer may be condensed into a very small space.

Indigo is a definite vegetable product, which appears to exist, in greater or less quantity, in a variety of plants, or rather the elements of which it is composed are found in these plants; for it is not until the juices of the vegetable begin to act upon one another in fermentation, that indigo is developed. It might at first be supposed that the green colour of plants was connected with the presence of indigo; but this is not the case, as after the leaves have been steeped in the vats, they retain entirely their original colour.

When first dissolved from the plant, the indigo is in a colourless state, and is readily soluble in water; but it becomes blue on absorbing oxygen from the air, and appears then to have

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assumed the nature of a peroxide, for it is very unalterable, quite insoluble in water, alcohol, ether, saline infusions, alkalies, and dilute acids. Concentrated sulphuric acid alone acts as a solvent, without changing its nature. Nitric acid converts it into a yellow bitter principle. Acetic acid by degrees deoxydizes it. Chlorine also destroys its colour immediately.

In saying that concentrated sulphuric acid effects no change upon indigo, I am not strictly correct. It does not destroy the colouring matter, or cause a decomposition, as would be the case with most other vegetable compounds; but the researches of Crum and Berzelius have rendered it probable, that three different modifications of indigo may be brought about by the agency of this acid, differing from one another in the quality of oxygen or of water in their composition.

The three modifications are thus distinguished:—

1. The *Pure Indigo*, which is obtained by sublimation from the crude cakes at a temperature of 550°. This is crystallized in long flat prismatic needles; has a copper colour by reflected, and a fine blue by transmitted light. It sublimes entirely at the temperature stated, without residue: its specific gravity is 1.35.

2. *Cerulin*, or *Saxon Blue*, is obtained by digesting the former substance in sulphuric acid: it is supposed by Crum to have lost its water of composition thereby. It is soluble in water, but is precipitated by most of the neutral salts.

3. *Phenicin*, or *Purple Indigo*, is obtained by suddenly diluting the sulphuric acid which has begun to dissolve indigo. It separates as an insoluble powder, which when filtered and washed, is soluble in pure boiling water, and may be procured as a precipitate again by the addition of any neutral salt. Phenicin is supposed by Berzelius to be an intermediate state between soluble and insoluble indigo; but Crum asserts that 100 parts of indigo will yield 120 of phenicin.

The indigo of commerce most probably ranks under one of the two latter denominations, or, perhaps, both of them; for there can be little doubt that some new combination of elements takes place in the sublimation of the "pure" or "crystallized indigo," since with the utmost care, not more than one-fifth of the weight of crystals can be procured; and, during their forma-

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tion, first, aqueous vapour, then gas is extricated; and a red coloured oil; and a large residue of charcoal is left behind; whereas the crystals, once formed, are volatilizable without any loss or carbonaceous residue. As the crystallized state, and the resistance to destructive agencies at an elevated temperature, are the sure signs of a definite, and generally of a simple atomic composition, we should expect to find tolerable accordance in the analysis of the crystals of indigo. I have only been able to find two made by different chemists, but they are greatly at variance with one another *.

Pure indigo, analyzed by Thomson		*by Crum.*
Consists of Oxygen	46.154	12.60
Carbon	40.384	73.22
Azote	13.462	11.26
Hydrogen	——	2.92
	100.	100.

The other varieties are stated by Crum to be thus composed:

	Cerulin.	*Phenicin.*
Oxygen	29.0	21.6
Carbon	57.8	64.9
Azote	8.4	9.5
Hydrogen	4.8	4.0
	100.	100.

If the composition given by Thomson be correct, indigo ought to be a remarkably easy substance to analyze; since it would merely be necessary to submit it to a destructive heat,—to determine the proportion of carbonic acid, carbonic oxide, and azote,—and to weigh the surplus carbon. The quantity of oxygen in the analysis of Mr. Crum will be proved hereafter, I think, to be too small.

From the almost total insolubility of pure indigo, it becomes a very simple matter to separate the foreign ingredients which are found with it, and consequently to obtain it in a state of purity. All that is necessary is to boil it for a sufficient time,

1st, In pure water, which removes yellow extract, green matter, &c.

* Dr. Ure gives in the Appendix to the second edition of his Dictionary, Ox. 14.3, Carb. 71.4, Az. 10, Hydr. 4.4. But he thinks these numbers may require a little alteration.—Ed.

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2nd, In alcohol, which carries off red colouring matter, and resin.

3rd, In dilute muriatic acid, which takes up lime, oxyde of iron, and magnesia, &c.

These methods will not, however, remove sand and clay, the presence of which can be detected by burning a portion of the blue cake.

The quality of indigo seems materially to have improved of late years, for Chevreul only obtained 45 per cent. of true colouring matter from Guatimala indigo; Bergman found 47; Brande states it at 50 per cent.; whereas in two specimens which I analyzed in Calcutta, one contained 75, the other nearly 80 per cent. of pure blue: neither were these the finest produce of the market.

Deoxidizing substances, such as the sulphurets, protoxide of iron, phosphorus, the sulphites, &c., have the power of depriving indigo of a part of its oxygen, whence it becomes again soluble in water or alkaline leys, preserving the power of regaining its colour the moment it afterwards meets with oxygen. It is thus that the dyers are enabled to prepare a solution for the purposes of their art. They are said to make use generally of sulphate of iron and slaked lime, which are mixed up intimately with the indigo, in the proportions of two of the sulphate, 50 of lime water, and 1½ parts of indigo, and then boiling the mixture in water. The colour of the solution is yellow.

Indigo thus deprived of its oxygen has been called Indigogene by Liebig: Berzelius calls it "Reduced Indigo." It is said to be obtained with facility in precipitation from the dyer's solution by muriatic or acetic acid, to which a small addition of sulphite of ammonia must be made, to prevent the access of oxygen.

Liebig asserts that this indigogene, at a moderate temperature, absorbs oxygen suddenly from the air with a species of combustion, and that in the mercurial eudiometer the absorption is found to amount to 11½ per cent. of its weight. The colour changes simultaneously from white to a rich purple.

Indigogene is soluble in the caustic alkalies and lime water, in which it may be kept without alteration for any period, provided air be entirely excluded. The solution in potash forms

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an excellent eudiometrical liquid, as it absorbs the oxygen of the air with great avidity, without giving out any gas to complicate the result. I had occasion to remark this circumstance in the course of the experiments hereafter described; and as the solution may be prepared direct from the manufacturer's vats in any quantity, it may prove a valuable discovery in the laboratory as a useful substitute for Sir Humphry Davy's eudiometrical liquid, composed of green sulphate of iron, saturated with nitrous gas, which is difficult to preserve, and may give out a little nitrogen in its operation. It remains, however, to ascertain how long the alkaline solution of indigogene will keep unaltered.

Having thus briefly enumerated some of the principal properties of indigo, as a substance *sui generis*, (and there appears to be no other vegetable product which resembles it in containing so much oxygen without being acid, and in the absence of hydrogen, and the presence of azote,) I shall proceed to the experiments on the process of manufacture, which form the immediate subject of this paper.

MANUFACTURE.

"The plant, after being cut and carried to the factory, is thrown into the steeper or superior vat, where it is pressed with timbers adapted to the walls of the vat, to prevent its rising in the water, which is then filled in from a reservoir, so as completely to cover the plant."

During the fermentation which follows, bubbles of gas rise to the surface, to ascertain the nature of which our first attention was directed.

The bubbles collected from the vats were found to contain merely 7 or 8 per cent. of carbonic acid; the remainder being common air, with from 12 to 18, instead of 21, per cent. of oxygen. Earthen vessels were inverted, and left with their mouth immersed all night in the vats; but the air in them was found unchanged. When bottles were partially filled with the liquor of the vat, and well closed, the air, after a day, was always found in them contaminated with about 18 per cent. of carbonic acid,—the rest being common air, without diminution of oxygen, excepting that portion due to the original air now replaced by the carbonic acid gas.

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By way of examining in a more unexceptionable manner, the gas given out during fermentation, the operation was conducted on a small scale, by steeping some of the leaves in a glass cylindric vessel furnished with stopcocks and tubes to convey the gas, which should be emitted, into a glass receiver.

After twenty-four hours, (for the process of fermentation does not proceed so rapidly as in a large vat,) the quantity of air given out by twelve sicca weight (= 2160 grs.) of leaves, was 26.1 cubic inches: the disengagement still went on, but very slowly.

The gas was analyzed at two different periods: towards the middle of the disengagement it was found to be composed of

Carbonic Acid . . .	27.5
Oxygen . . .	5.8
Azote	66.7
	100

And at the conclusion it contained—

Carbonic Acid . .	40.5
Oxygen	4.5
Azote	55.0
	100

Probably the atmospherical air of the apparatus, as well as that contained in the water, and that which remained entangled among the leaves, may account in some measure for the presence of the oxygen and azote in the first analysis; but the large proportion of azote, especially in the second experiment, so much surpasses what might be expected from this source alone, that it seems necessary to attribute it, in part, at least, to an emission from the leaves during the fermentation; or probably the leaves, still retaining atmospheric air on their absorbent surfaces, convert the oxygen thereof into carbonic acid, and allow the azote to escape. After the disengagement has ceased, it will be seen by the next experiment that the proportion of azote decreases very much. Some of the fermented liquid of a large vat was well boiled, and the gas collected over water: on analysis, it proved to be composed of—

Carbonic Acid . . .	78.
Oxygen	2.3
Azote	19.7
	100

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And in a second experiment, conducted with greater care to exclude external air, the result was 86 per cent. of carbonic acid, and the residue contained too feeble a portion of oxygen to explode with the electric spark on the addition of a requisite proportion of hydrogen.

In no case, therefore, does the extricated gas in practice appear to be pure carbonic acid; but from the prevalence of the latter increasing with the precautions taken to exclude common air, it may in fact be the only gas strictly due to the fermentation properly so called, the remainder of common air having been suspended in contact with the leaves, and being deprived of a portion of its oxygen by the free carbon, or by the liquid in its passage to the surface. No carburetted hydrogen or other combustible gas was found among the gaseous products.

The indigo manufacturer does not wait until the extrication of gas is concluded, but withdraws the liquid from the steeping-vat as soon as he considers it to be sufficiently fermented; judging either from the smell, from the greenish tint of the liquor on the surface, or from the formation of an iridescent scum on the bubbles of gas. In fact, when the liquid, which is of itself of a bright yellow colour, begins to assume a greenish tint, it is evidently caused by an incipient precipitation of the blue colouring matter; and it would be attended with a loss of produce, to allow this precipitation to take place in the steeping-vat among the leaves and branches of the plant.

"The length of the fermentation depends upon the temperature, the weather, the wind, the water employed, and the ripeness of the plant; it may last in common cases from seven to fifteen hours. It is generally longer when the temperature is high, the weather cloudy, but not rainy; the wind eastward and moderate, the plant ripe and fresh."

Upon these several points, constant experience leaves little for the experimentalist to advance; but it may be remarked, that almost all the criteria of a good fermentation, as far as the weather is concerned, concur in one simple principle;—the prevention of the water of the vat from being cooled; for the *west wind*, being dry, cools it by evaporation; strong wind does the same; clear sky cools it by radiation; and rain, by the low temperature of rain-water. Thermometers placed in

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the vats did not present any satisfactory results; the mean temperature was about 85 degrees Fahrenheit.

"The fermented liquid is drained off into inferior vats, which are called the 'beating vats.'"

At the time when the vat is opened, the liquid is found by experiment to have a specific gravity (at the surface) of 1001.5, and at the bottom of the vat, 1003.1. The leaves appear to have lost nothing, being as green and fresh as when they were first strewed in the vat. By carefully weighing a portion of leaves, however, before immersion, and washing and drying them in the air after it, they were found to have lost more than three quarters of their weight: of this loss the greater part was water, which they apparently cease to have the power of retaining when the soluble juices have been withdrawn. The solid matter taken up by the vat amounts to between 12 and 14 per cent. of the weight of the leaves.

When the fermented liquid runs off into the lower vat, a frothy extrication of gas covers the whole of its surface. It is a good sign if the froth, in subsiding, assumes a rosy tint, which is in fact nothing more than a very thin film of indigo, and it proves that the deposition is ready to take place.

"In this vat the liquid undergoes a beating for about two hours—it is continually stirred about and agitated by a number of men, either with their arms or with a sort of short oar."

The object of this operation appears to be threefold. In the first place, the agitation extricates a large proportion of the carbonic acid gas which still remains combined with the liquid:—in the second, it exposes fresh surfaces continually to the contact of the air, whence the oxygen is rapidly seized by the nascent indigogene:—and thirdly, it coagulates the fecula of the indigo in larger grains, so as to render it more easily precipitable.

By way of understanding more clearly what takes place in the beating vat, a number of bottles were at different times carefully filled with the yellow liquor, just as it was ready to be drawn off from the upper vat, for experiments in the laboratory.

Neither keeping, boiling, the addition of acids or alkalies, nor even putrefaction, appeared materially to affect the power

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of depositing Indigo,—the solution always became blue the moment it came in contact with oxygen.

It may be remarked, however, generally, that the longer the liquid had been kept, the less rapid and determinate was the deposition:—the fecula remained in part suspended in the liquid, giving it a green hue; but in time it invariably subsided, and the quantity appeared to be the same in all cases.

It is sometimes customary in the beating vat, when the precipitation does not proceed with vigour, to throw into the vat a little lime water, or some other precipitant, to assist the Indigo in subsiding; the effect of such additions was tried on a small scale, taking care to exclude the air during the immediate application of the re-agent.

The acids and carbonated alkalies caused an immediate extrication of carbonic acid gas from the liquid, but produced no precipitate.

The caustic alkalies and lime, on the contrary, produced a copious deposit, unattended with effervescence. The colour of the deposit was yellowish-white, if the air was quite excluded; but it became green and blue, upon the slightest contact with oxygen. Careful experiments, however, proved that the blue colour was only produced by the indigo attaching itself to the precipitate; for all of the indigogene, or vegetable matter convertible into indigo, remained suspended in the supernatant alkaline liquid. The precipitate was composed of a yellow extractive matter, to which I shall again advert presently.

The measurement of the absorption of oxygen during the transition from the colourless to the blue state, was the next object of experiment. It was easily put beyond a doubt, that such an absorption took place; but several trials to measure it failed, on account of the extrication of carbonic acid gas, which was always much greater than the oxygen absorbed.

I thought that boiling would have driven off all the carbonic acid; but I was astonished, on filling an eudiometer bottle with liquid which had been entirely purged of all its free gas by ebullition, to perceive that the moment the tube containing 100 measures of oxygen was connected with it, a brisk emission of carbonic acid (about 50 measures) took place, and confused the results.

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At length, by adding a little caustic potash to absorb the carbonic acid which might be generated, the diminution of oxygen became apparent, and in a few minutes all the oxygen contained in the eudiometer tube disappeared, the tube being too small to supply enough for saturation.

As the employment of potash was on some accounts objectionable, I also tried and succeeded in another method of obviating the presence of carbonic acid.

In a glass balloon, furnished with a stopcock, a vacuum was made, into which the liquid was suddenly introduced by a connecting tube. By this means a great deal of gas was separated, and, by repeating the action of the air-pump, much of what remained was withdrawn.

To the vacuum above the liquid, pure oxygen was admitted from a mercurial gasometer, its quantity being measured; and the air in the balloon was further analyzed by withdrawing a small portion. The balloon was then agitated, and water from time to time admitted to replace the oxygen which had been absorbed. The residual gas was again analyzed, to find whether the carbonic acid or azote had altered in quantity during the experiment, and the weight of the oxygen absorbed was ascertained from that of the water which had entered the balloon.

Four experiments conducted in this manner, yielded the following results:—

	1 *Expt.*	2 *Expt.*	3 *Expt.*	4 *Expt.*
Weight of liquid grains,	8455	5500	5166	6478
Weight of oxygen absorbed,	2.00	0.82	0.84	1.22
Weight of Indigo collected,	6.8	2.9	3.9	4.9
Proportion of oxygen in the Indigo per cent.	29.4	28.2	21.5	25.0

Making an average of 26 per cent. of oxygen absorbed; a quantity nearly double what Crum supposes to exist in pure indigo, and an average between that which he ascribes to cerulin and phenicin. It is, however, little more than the half of what Thomson states in his analysis.

There is hardly any occasion to seek a nearer concurrence between experiments made in the manner just described on the liquid of the vats; because the indigo forms but a very small proportion of the matter held in solution therein, and it is

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very possible that the other ingredients may also absorb oxygen, especially when the putrid fermentation commences.

It is evident, however, that a further analysis of pure indigo in the dry way is a very desirable object. It seems to have escaped the attention of Gay-Lussac and Thenard, and latterly of Marcet and of Dr. Prout, in their Researches upon the Constituents of Vegetable Compounds.

It is worth while in this place to mention a fact observed in the course of these experiments, namely, that where a slight excess of potash is mixed with the vat liquid, the indigo formed remains in solution, and passes through the filter with ease, leaving the precipitated extractive matter behind. This blue solution will keep for any time, and does not deposit its indigo even in the open air; but as soon as the alkali is saturated by an acid, it immediately precipitates, leaving the liquid colourless. If the dose of potash be not sufficient to throw down all the yellow matter, the solution retains a green colour, and cloth dipped into it takes a green dye. When hung up to dry, however, the mixed action becomes evident; for the dye remains fixed in the lower part of the cloth, while the yellow, more perfectly dissolved, spreads with the liquid to the upper part which was not dyed. There is no reason to believe, in this case, that the air changed the colour from green to blue, although such may be true of the dyer's vat-liquid.

The quantity of indigo deposited *per se* from 1000 parts of yellow liquid of the specific gravity 1003.1, varied but little, and may be estimated at 0.75 parts. In practice, a vat of 637 cubic feet is considered to yield a good produce at 16 seers, which is as nearly as possible 0.75 per 1000 of liquid. The common produce of the vats in this part of the country does not exceed 0.5, or one five thousandth of their weight of indigo.

But when potash, lime, or other precipitants are employed, the weight of the indigo is much increased; not, as experiment proves, from an increase of the actual produce, nor from any union of the precipitant with the indigo, but from its causing the deposition of another matter, to which I have given the name of yellow or brown extract.

To obtain this yellow extract in an insulated state, 10,000 grains of mother liquid were evaporated to dryness: a solid

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residue was in one case collected, weighing 47 grains. With another liquor it was 20 grains heavier; and in the experiment made on a small scale with the leaves, where the fermentation continued for 24 hours, the same weight of mother liquor yielded 246 grains of residue. As this element of the plant seems to be so variable, and as it must evidently produce much influence in the manufacture, it is probably one of the chief causes of the nicety usual in timing the fermentation, and of the variable tendency of the indigo to precipitate in the beating vat. The dried extract has a dark-brown colour and vitreous surface, similar to that of dried gluten, or the brown extract of toast-water: it has a peculiar, not unpleasant smell, and rather a bitter taste: it deliquesces in a damp atmosphere, and dissolves in water, which it dyes of a deep brown or bistre. Although the original mother liquor is exceedingly liable to ferment and become putrid, the brown matter undergoes no change, either when kept dry, or in a moistened state.

It is precipitated from its aqueous solution by potash, soda, ammonia, lime, and their carbonates; by infusion of galls, acetate of lead, and nitrate of silver. The acids, and prussiate of potash, did not affect it; but the action of re-agents was not investigated in detail.

Sufficient has been adduced to prove, that whenever lime or the alkalies are used in the vat, the indigo must be adulterated, more or less, with this substance; and I suspect that the brown and green matters, separated by Chevreul and others, in their analysis of indigo, are attributable to this source. In an experimental way, indigo of a true dark green colour was easily collected, which weighed more than twice as much as the blue colour alone: it was also harder and more compact, and more liable to shrink and crack than the pure indigo cake; for besides the impurity just described, there was always found a large proportion of earthy residue, on burning indigo, where a precipitant was used, than where it was not; the quantity was even as much as double or triple in amount.

Carbonate of lime, alumine, and oxide of iron, are the chief ingredients of the earthy residue, and I have seen them vary from five to nearly fifty per cent. of the indigo: the last, as may be supposed, was merely refuse, and quite unsaleable.

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Carbonate of lime is seldom absent; for whatever lime may be in the plant, or in the water, is sure to be precipitated by the carbonic acid developed during the fermentation. Some manufacturers are in the habit of employing rain water purposely to avoid this source of adulteration, but it is doubtful whether the plant will not itself bring a portion of earths into solution.

Upon the whole, it may be safely decided, that the purer the water is, and the cleaner and the more simply all the operations of the manufacture are conducted, the more beautiful and rich in colour will be the indigo: and it should be the first maxim of the planter, that it is purity, and not weight, which gives the value to his produce in the market.

In conclusion of the present desultory notice, which will, I hope, at least serve the purpose of leading others to bestow further attention upon the subject, I annex an analysis of a specimen of indigo, denominated fine blue in the Calcutta market, made in the year 1820, to which I have referred in a former part of this paper.

Analysis of Calcutta Indigo.

100 grains heated white in a closed platina crucible left a porous grey carbonaceous mass, with metallic lustre, weighing . .	49.0
Burnt with access of air, the 49 grains were reduced to	7.42

1.—*Examination of Earthy Residue*, 7.42.

1. Boiled dry in nitric acid, and then digested in muriatic acid, a brown residue of oxide of iron and alumine remained .	2.7	
2. From the solution, ammonia threw down alumine . .	0.75	
——————— Oxalate of ammonia—lime equal to .	0.9	
3. The clear liquor evaporated left red oxide of iron . .	3.05	
		7.4

2.—*In the humid way.*

1. 100 grains of indigo digested in boiling water, some green and dark brown matter was dissolved, which, when dry, weighed .	1.6
2. Alcohol boiled over the remainder became of a bright claret colour, and yielded, on evaporation, a dark brown matter and a little yellow resin, weighing	2.0
3. Dilute muriatic acid then took up a mixture of greenish vegetable matter and earths, which were afterwards separated by burning. The green matter was thus found	7.2
4. The indigo now deemed pure weighed only	79.5

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This sample of indigo, therefore, was of the following composition:

Oxide of iron	5.75
Alumine	0.75
Lime	0.90
Green vegetable matter . . .	8.80
Red or brown ditto	2.00
Pure indigo	79.50
Loss	2.30
	100.00

Mr. Burnett *on the Functions and Structure of Plants: with reference to the Adumbrations of a Stomach in Vegetals.*

To the Editor of the Quarterly Journal of Science.

Dear Sir,

"*Was not the first animal that ever lived, a plant that found out the blessing of a stomach, and ran away with it?*" Such was a proposition suggested to me after the Physiological Lectures I gave at the Royal Institution last spring, in which the form and functions of the various organs of the vegetable frame, and the distinctive characters of the several grades of the organic realm, had formed the subject matter of discussion.

This was a hasty conclusion, and one certainly not warranted by the premises adduced; for although the stomach is confessedly a most important organ in the animal economy, an organ to which we scarcely find anything analogous in plants, and locomotion one of their most valuable endowments, still neither are constantly present; and even were they, sensation, true instinct, and volition would yet remain, the best diagnostics of the animal creation. But there is something peculiarly terse and epigrammatic in the idea which would thus boldly, and in so few words, enunciate two of the most celebrated definitions of a plant, and include their necessary interdependence; for physiologists have long dwelt on the circumstance, that "vegetables are nourished by their external, while animals are nourished by their internal surface;" and naturalists have frequently referred to the power of "moving from one place to

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another" as the *most distinguishing characteristic* between brutes and plants. Age after age have these observations been repeated and re-echoed in almost every diversity of form;—thus Aristotle designated plants, *animals turned inside out*, while Jungius, Ludwig, Ray, and others, ringing their successive changes in scarcely differing words, define plants to be "organic bodies fixed to a certain place whence they are nourished and encreased;" or, as Boerhaave and Martin state the question, "adhering to another body in such a manner as to draw from it their nutriment:" and hence Linnæus, as the Stagirite had done before him, described a plant, and not inaptly, as *an inverted animal*. These definitions, though at first sight plausible, are too superficial, admit too many exceptions, and are clouded by too many obscurities, for the rule ever to have been made absolute, even as a popular test; and once gave rise to a repetition of that practical sophism, which it is said was long since enacted with regard to Plato's famous definition of a man, viz. "that he is an unfledged biped;" upon which the Cynic philosopher, having plucked a fowl, is reported to have exclaimed, "Behold Plato's man!" And thus the simple sacculate polypes have been turned inside out, and then the question asked, whether such an inverted animal had become a plant.

Continual reference we find both is and has been made, to the roots and absorbents of the vegetable body being external, *i. e.* distributed without itself, for plants even when vagrant as the Lemna of our ponds, and the Confervæ of our lakes and streams, still are in communion with their peculiar site, be it earth, air, or water; *i. e.* as Link observes, "deriving nourishment from the soil in which they grow."

Many physiologists hence would explicate the problem, why locomotion is so commonly the privilege of animals, and as necessarily, in general, denied to plants. For as life is alone sustained by the constant reparation of that machine which its actions as unremittingly impair, those beings which depend for this uninterrupted renovation upon supplies ever situate without themselves, must consequently be held in uninterrupted connexion with their external food—that is, with the soil in which they grow; whilst those which can intuscept their food (upon which they in like manner grow) are unrestrained to

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especial place, and may be truly said to bear about their soil within them.

Thus the earth has frequently been referred to as the common stomach of plants, and the decompositions which therein take place have been loosely likened to digestion; the roots of the vegetable body being considered analogous to the lacteal ducts of animals, for these absorb and convey as chyle, the eliminated extract of the one, as those imbibe and transmit, as sap, the juices furnished by the other. But the earth is not the external stomach of plants any more, nay, not so much as the nest is the external uterus of birds: the analogy is greatly forced; and the parallel would have been much closer, had the earth been described as the kitchen, rather than as the stomach of vegetables; fermentation, putrefaction, and the other changes which therein go on, being much more similar to the processes of cookery, than to the several stages of digestion. In truth, the lymph absorbed by the roots is still crude aliment, nor does it become changed into proper sap until after it has been elaborated in the leaves or other assimilating organs of the vegetable frame; the digestive, circulating, and respiratory functions being all of them more or less combined in plants, and never so separate in their organic systems as in brutes and men.

My present object, however, is not to shew the insufficiency either of locomotion or a recipient stomach, as diagnoses between animals and plants; for *Conferva*, *Volvox*, the *Corallines*, &c. have long certified that point; but rather to trace the slight adumbrations of a stomach that we find in plants; an organ so all-important, all-engrossing to animals at large, that to cater for its due supplies their other members are so chiefly, so constantly employed, that one might almost be disposed to think, that they do not "eat to live, but live to eat:" and which anticipations, though faint, are curious; as far as I know, they have not hitherto been dwelt on, or at any rate would seem too slightly to have been passed over.

Nature, ever fond of working on the same model, seldom arrives at any new form suddenly or *per saltem;* but in her progress towards perfecting a type, by scarcely perceptible degrees modifies the most diverse organs, prefiguring, for many

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successive grades, a change, which, subordinate when first perceived, subsequently becomes of paramount importance. Every function, or system of functions, upon the due performance of which vitality depends, will equally attest this fact, and with equal truth; for whether we examine the nutritive or the reproductive apparatus, the digestive or the respiratory systems, the organs of sense and motion, or the phenomena of irritability and instinct, each will witness, as each will furnish the examples, that the progressive rise of organic beings, whether animals or plants, consists rather in the separation and perfection of the respective functions, and the organs which they severally possess, than in many distinct endowments allotted to their various grades—rather in the perfecting those principles at first laid down, principles common to all, and by the enjoyment of which all subsist, than in any new mode of being peculiar to different kinds. The modifications of these general principles are numerous and vast; still the same plan is evident, the same rule governs, the same principle prevails; and as in one so in all, the self same dogmas uniformly guide, and every where direct the whole. Thus the humble moss and fungus, the lowly conferva, the most simple protophytes, differ not so much from the giant oak, the lordly palm, or the irritable mimosa, in what they do, as in the manner their essential duties are performed: similar ends are achieved by each, though diverse instruments are frequently employed. They are each endowed with organs of nutrition, by which they support themselves; they are each possessed of organs of reproduction, by which they perpetuate their kind; but in the one the absorbents pervade the whole structure, or overspread the entire surface, so that every part is equally and indistinguishably root: and again, if torn into many pieces all the parts will grow, for all alike are germinating points, each having the essential powers present in every part; while in the other series the nutritive system is more or less separated from the reproductive, the one exhibiting a proper root, the other a complex flower and seed. This separability of function, and distinction of structure, for the more or less perfect evolution of the various organs, and concentrating the power and energy of each, is a subject well worthy consideration: the grades are not less curious in animals than

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in plants; nor more so in the one than in the other: let an example from either reign suffice for illustrations.

Take the freshwater polype (Hydra viridis or grisea) a small lump of translucent jelly, about the size of a pea when contracted, but when extended, and viewed under favourable circumstances, lengthened in its body to about three-quarters of an inch, and more resembling the finger of a small glove, with a few ravelings round the edge, than any other familiar figure. This creature possesses neither wings nor legs, nor any of the ordinary organs of progression; it is apparently homogeneous in its structure, shewing not even a rudiment of bone for leverage, or a semblance of muscle for contraction, and yet it protrudes and withdraws its tentacles, moves from leaf to leaf, travels from plant to plant, from stone to stone, quits the dark and approaches the light side of the vessel in which it may be kept, basks in the sun-beams, enjoys the warmth of summer, becomes torpid during cold weather, and hybernates like the tortoise or the dormouse; retreats if touched, defends itself when attacked, and often attacks in turn; pursues its prey with avidity, and, although it has neither tongue, nor teeth, nor palate, yet with hungry relish it devours the minute animalcules it can catch; nay, even with cannibal propensities, will force smaller or weaker individuals of its own species into its simple pouch or stomach, digest a part, and then reject the fæces by that single aperture which is both entrance and exit, both mouth and vent to this gastric prototype, which thus absorbs a part of its ingested food and vomits up the rest; such being the natural process in this simple being, to which the higher grades return in many cases of disorder or disease. And yet so finely does this *prima communis via* participate in the peculiarities of digestion, and acknowledge its general laws, that, like the animal stomach of the highest grade which will digest a bone when dead, but cannot act on a pulpy worm when living, this pouch can only feed on prey that has been truly killed. Trembley, I think, it was who observed two hungry polypes fighting which should become the other's meal; or perhaps the little one endeavouring to escape, the greater attempting to devour the less: strength, however, at last prevailed, and this Saturnian polype swallowed

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at one gulp his son: the little fellow, not being, however, slain, was indigestible, and played such freaks within his living tomb, that the greater one, quite sick at heart, returned his dinner, unhurt, uninjured, to the light of day. But again, the polype has neither eyes nor ears, nor any of the ordinary organs of our senses, and yet it sees and feels, or at least is sensible both to light and touch, and probably to odours and to sound. Every part of this thing's body is equally sensible to the various stimuli which affect its system; it is an eye, an ear all over, but of what a kind!—an eye which sees not, an ear which does not understand: and when vision is to be perfected, the visive function becomes isolated, and the power concentered to a peculiar organ, which is developed by degrees to its highest point; and as of the eye, so of the ear, the hand, and all the rest.

In like manner, as among animals, so also among plants, a division of labour, and separation of systems, leads to the perfecting of organic structure, and of functional achievements. The Tremellæ and Oscillatoriæ are closely analogous in many respects to the polypes just referred to, and hence may form a parallel illustration. These simple gelatinous existences, known to the vulgar as "fallen stars," or sleeping "Wills o' the Wisp," are so ambiguous in their simplicity, that the learned doubt whether they should esteem them animals or plants; but as they possess not locomotion, nor exhibit any of the pugnacious and other qualities of the polype, which in higher animals are known to be the offspring of volition, sense, and instinct; and as the doctrine of irritability will sufficiently explain all the phenomena which they evince, it is more philosophical to consider them as plants. These slimy or gelatinous Tremellæ nourish themselves, as seems proved by their assimilated increase of size; they reproduce their kind as seems witnessed by their consecutive duration; yet they have nought that can be truly called root, or branch, or leaf, or flower, or seed. Many of the Confervæ are vagrant, unattached to any spot, and they, as well as the generality of the Fuci, even when fixed to cliffs, or rocks, or shells, are so adherent, or rather adherent to such substances, that the one can afford, the other derive, no nutriment through what would seem their root, but absorb their food by the whole of their frondescent structure.

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The Fucus natans, the Conferva vagabunda, C. Ægagropila, &c. are not only decidedly nourished thus, but are also unattached and wandering plants, vegetable *vagabonds* as their names import, being apparently destitute of root; while the Tuber cibarium, or truffle, on the other hand, would seem to be wholly root, it has neither stem, nor leaf, nor flower, nor fruit; and yet as the former are nourished without a root, so in the latter the functions both of respiration and reproduction are essentially and effectually performed without even the semblance of those organs which are elsewhere their peculiar sites. Thus, in one type, *e. g.*, the Tremellæ (nostoc), we have nought distinct of any organ, or root, or stem, or leaf, or flower, or seed; in another, as Tuber (cibarium), they are all combined under the likeness of a root; in the Confervæ and Fuci sometimes the similitude of leaf, sometimes of stalk, would seem chiefly to prevail. In Testudinaria, the intermediate caudex (in most plants so obscure) predominates; while in Aphyteia and Rafflesia little else, save flower, is known. In the rushes and the cacti, the leaves are latent in the stem; in the lichens and the ferns, the reproductive germs are hidden in, or united with, the leaf-like fronds; while in the more common and most developed plants, the root, the stem, the leaf, the flower, the seed, are all distinct; the root absorbs, the stem upholds, the leaves respire, in short, each segregated organ is found fulfilling its especial duty; and neither performing, nor fitted to perform, the function of another: and yet in some, an occasional relapse is made in one or more particulars towards that primitive universality of system, whence, after repeated efforts, nature has extricated her superior works; which relapses are the intermediate links that establish an uninterrupted connexion from the highest to the lowest grades. Thus floral germs will grow, as in proliferous flowers, in continuation with the parent stem, returning to the character of buds; and gems occasionally will loosen, and assume, in part, the functions proper to seeds. Leaves and stalks will radicate at many points, and by art be separable, or often separate spontaneously, into many independent plants; or, even without this violent discontinuity ensuing, absorbents will detrude themselves on the branches and trunks, even of forest trees, or pervade and cover

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an entire plant, so as to remind one of the primitive ubiquity of root; when, as in the protophytes, the nutritive and reproductive organs were mutually and indistinguishably blended with each other, forming but one common yet effective whole.

"It is matter of notoriety that plants, even what we consider the more perfect kinds, derive nourishment from the atmosphere as well as from the soil; and this in very different degrees: that some depend much on this, some much on that; and again, in others it is directly the reverse; that some plants will flourish on very poor soils, and others wane in comparatively good ones; that thistles will grow and enrich a plot where corn would degenerate and die. The turnep husbandry has practically familiarized this to every one, and the farmer well esteems the rest, to say the least of it, if not improvement, which such a crop affords to land, while wheat impoverishes it greatly. Experiments have proved that plants, with few and small leaves, depend almost entirely on the resources of the soil, those with many and large ones almost as exclusively on the atmosphere around them; the mosses, and most of the pseudo-parasites, for example: and some of the Epidendra, as the Flos aëris, will grow while hanging from the boughs of trees into which no roots are sent, or even, if suspended by a cord from the cieling of a room, will produce leaves and flowers for months or years together, nourished only by the elastics floating in the air. In some of the simpler tribes, the stem or entire plant, and in others, both sides of the leaves, or leafy appendages, equally absorb nutriment from the water or the air; others absorb chiefly or only by a single surface, which sometimes is the under, sometimes the upper page; and when, as in the connate leaves of Dipsacus, the hollowed basin of Hydrocotyle, or the follicular appendages of some rare plants in which a sac is formed by the intorsion and coadunation of their leaves, in which water and other matters are not only lodged and retained, but can be thence absorbed by the vessels of the plants which are spread therein, a considerable advance has been made in these pouch-like reservoirs towards an internal cavity for the reception and retention of food.

"In many plants, in which concavities are found of various

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shapes and sizes fitted for the retention of water when caught from rain or condensed from dew, the cups or sacs would seem to be fortuitous appendages; or rather anticipations of 'coming events which thus cast their shadows before;' for the water, when caught, seems not necessarily applied to any important use in the internal economy of the plant, which still depends on its terrene root for its support. In other instances, however, (in perhaps the Teasel, and certainly the Pitcher plants) the reverse is found to be the case, and a great advance is here perceived towards a digestive receptacle, over those simple plants which not only are destitute of such foliar appendages and sacs, but also are devoid of leaves; or even over those which having leaves, still are chiefly dependent for support upon their ordinary roots. These various examples which mark the progressive stages of developement in the organization, and the gradual separability of structure in perfecting the functions, even of automatic life, although unworthy a more formal essay, may probably be thought not undeserving this transient notice.

In Nepenthes and Sarracennia, previously referred to, this shadowing forth of an animal organ is carried to the furthest developement that occurs among plants; in these and other similar vegetables some of the leaves are of the ordinary types, while others are so congenitally incurved and connected at the edges as to form a pouch or pitcher of considerable extent, or the membranous sac is even an especial appendage separate from, although a continuation of, the midrib and proper expansion of the leaf. In many instances this apparatus is fitted with a lip or lid, by which the mouth can be shut and opened; the machinery of which limb is so contrived that, when the cavity within is well supplied, it closes to prevent evaporation; that when the stock is diminished or consumed, the lip is raised, so that the mouth again is open to receive the falling rain, or the rising dew. That such plants condense into their receptacles large proportions of atmospheric moisture, as other plants do on their general surface, common observation will sufficiently attest, for they continue well supplied, even in conservatories, where no rain can reach them; and in many, the over-hanging lid forbids the entrance of falling moisture, but its arched form is well fitted to receive and condense the

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rising vapour: and it has been noticed that these pouches become fuller of liquid after the neighbouring plants have been watered, or the floor of the hothouse has been wetted, although none has been supplied to those in question; and the inner sides of these cavities being downy, or often thickly set with hairs, are thus by their structure well fitted to condense the aqueous vapours which float around them; and the throats of those which cannot closely shut their mouths are frequently so contracted, as, in like manner, to retard the evaporation of the water they contain. Still these plants, although thus curiously constructed, depend greatly also on the soil for their support; and hence, their stems are unlike the stems of the attached polypes, (Madrepora, Millepora, &c.) which rather resemble the Fuci, in that the organ of their attachment is not the organ of nutrition, for, although adherent to a certain spot, the stem by which they are fixed is a mere fulcrum, and not in anywise a *root;* their absorbents are confined to their upper members, and, as is probably the case, with many Hydrophytes, chiefly disposed on one surface only. In some of the simplest monads, in which no cavity can be traced, the outer surface must be the seat of absorption, and one great effort, in animalization, is the hollowing such a gelatinous point, so as to form a cavity which may represent a stomach; many animals there are, as the Hydatids, which are wholly or little else than stomach. In the polype this pouch, simple as it seems, is, notwithstanding, considerably advanced; still it is a digestive system only, for the respiratory, circulating, sentient, and reproductive, are all in embryo; the entire surface is both skin and lungs; there is neither heart nor vessels, nothing like a brain or nerve, never an organ, or for sense, for instinct, or volition: and even if cut into twenty or fifty pieces, each will become an entire being, possessed of all the faculties, and practising all the arts which signalised its adult parent of which it was so minute a part. This indestructible divisibility of many of the lower animals which thus propagate by buds, by cuttings, or by offsets, was familiar to the vulgar long before the matter was dreamt of by philosophers; and when Trembley was experimenting on the Asterias and other radiate animals, the fishermen, who saw him cutting them in pieces, jocosely observed, "*Il perd son temps, il ne peut pas tuer ces choses.*"

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Just so, the entire frond at first absorbs nourishment in plants, afterwards particular parts only; in some advanced instances the root chiefly, in others the leaves also; in some both pages of the leaves, in others one side only; and in some only particular parts even of the selected surface. In most, where the leaves are feeders, they form organs of absorption only, but in some few cases they become organs for the retention of food also. And here another circumstance requires our attention, for in these receptacles, especially of Sarracenuia and Nepenthes, are generally (almost invariably) found flies and many small insects, which tempted to enter, either by the fluid itself or the excretions from the plant, often of a saccharine nature, with which it becomes mixed, are denied egress, either by the closing lid, its vaulted form, the contracted throat, or the bristly barrier before alluded to; the hairs of which being all pointed inwards, like the entrance to an eel-weir, or the wires of a mouse-trap, may easily be passed in one direction, but not in the other. The prey is thus entrapped and held, just as by the teeth of fish and other animals, frequently situated, not only on the tongue and palate, but also in the throat and stomach, being, like the hairs in these plants, organs of retention, not of mastication for their food.

It has often been objected to as an act of cruel amusement, if not of sheer malevolence, on the part of Nature, to set these vegetable fly-traps, as in the Dionæa, Drosera, Sarracennia, &c., to ensnare and destroy the heedless flies, shortening their already ephemeral existence; but observation and experiment would rather lead to the conclusion, that such sacrifices of the smaller insects form no unimportant items in the food of certain plants. In the pouch of one small Sarracennia, examined a few days ago, I found twelve common flies, and two woodlice; and the multitudes imprisoned and destroyed by the Dionæa and other plants would lead one to believe, were it from their number only, that Nature could never sanction such a waste of animal life, were it not to answer some important end in the well-being of those plants she has furnished with these organs of destruction. The little sundew which grows commonly in our bogs and marshes, abundantly, near London, on Hampstead Heath, would itself furnish suffi-

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cient proof. I have often seen several flies or worms in the possession of one of these small plants, which was flourishing by its prowess, and fattening on the delicates it had caught. But in the Sarracennia the number is still greater, often, in the larger plants, so great as to cast a strong and offensive effluvium around. The decomposition, however, is a necessary process; and it is, probably, both modified and checked by the saccharine secretions of the plants, which, like the gastric fluids of the animal stomach, may be fitted, not only to digest, but also to retard or regulate the putrefaction of the ingested food. To pursue the parallel, Rumphius has observed that, within these pouches, a certain small squilla or shrimp, with a protuberant back, is sometimes met with which lives there; so that even this simple digestive apparatus is not free from intestinal worms. Other more serious diseases would seem likewise not unfrequently to prevail, for the discoloured spots in the pouches of Sarracennia indicate serious disorganization, and the powerful and rapid decomposition of food, when taken in too great abundance, may fancifully be likened to indigestion from repletion, and then the occasional offensive odours may perhaps be symptomatic of vegetable dyspepsia.

The water in these receptacles, impregnated by the half-decomposing animal matter, doubtless affords a highly-nutritive and invigorating diet to the plant, for it is well-known that the drainings of dunghills give a very powerful stimulus to vegetables, as the rainwater that percolates there-through dissolves and carries with it, in solution, much of the nutritious and more subtle ingredients of manure; and as the food of plants is chiefly, if not wholly, absorbed in a fluid state, the more soluble manures are ever the most conducive to their growth. Nor must the nitrogen thus afforded to the prehensile plants be overlooked in the account, when we know how potent an excitant ammonia is to the vegetable frame. These speculations would seem, in some measure, to admit of experimental proof, for the Sarracenniæ, if kept from the access of flies, are said to be less flourishing in their growth, than when each pouch is truly a sarcophagus; and further, I remember to have either heard or read of a physiological experiment made on two plants of Dionæa Muscipula, selected for this

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purpose, of nearly equal size and health; both were kept under similar circumstances, save that the one was restrained from flies, worms, and all kinds of animal food, while the other was daily fed with small strips of rump steaks; the result of which experiment was, that the Epicurean plant languished on its lenten diet, whilst the beefeater flourished on its more substantial fare.

The plants to which I have alluded being furnished, some with organs for the prehension, and others with pouches for the retention of matters that are or may be used as sustenance, perhaps approach the nearest in their structure to that involution and appropriation of a part of their surface, for the especial purpose of retaining food, and absorbing thence its nutritious particles, of any that we at present know; and, consequently, are the nearest approaches, the strongest adumbrations (slight as they are) that we meet with in the vegetable kingdom, of an organ so prevalent among animals as to have been thought their peculiar characteristic, viz. a *stomach*. The gradations of this prime nutritive apparatus, both in brutes and plants, is curious, from the general use of the entire surface to the special ordination of a particular organ, which organ becomes progressively more or less involved, as it is more or less essential and important; as in the higher types almost all the systems, whether nutritive or reproductive, ordinarily are: and yet the original external cutaneous digester, even in man, the highest grade of all, has not lost entirely the original community of function that signalized it in the lowest types; for it not only perspires and absorbs matters hurtful and salutary, but likewise forms carbonic acid with the oxygen of the air, being in some slight degree an organ of respiration, an external lung, as it is also a vestige of the external kidney, stomach, and intestines.

On the peculiar properties and powers of vegetable digestion, whether the apparatus be spread over an entire surface, or collected into an especial organ, I have purposely avoided to dilate; and yet this is a subject of most curious interest, one to which hereafter I may probably, if time permit, return. Mirbel acutely noticed, that "Plants alone have a power of deriving nourishment, though not, indeed, exclusively, from

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inorganic matter, mere earths, salts, or airs, substances certainly incapable of serving as food for any animals, the latter only feeding on what is or has been organized matter, either of a vegetable or animal nature; so that it should seem to be the office of vegetable life alone, to transform dead matter into organized living bodies." This is a most philosophic and important observation; one which would further lead to the investigation of the problem, whether plants, in any case, *produce* those alkaline and earthy substances which are mediately or immediately peculiar to them, and whose metallic bases are chemical elements in our present state of knowledge; but the mere allusion to this extensive and disputed topic is almost without the scope of the present trifle, already stretched unduly in its length.

To conclude, hence perceive we that the definition of Aristotle, though incorrect, was, for the age in which it was adventured, one of extraordinary astuteness, when he said that "plants are animals turned inside out;" for Linnæus, who repeated this observation many centuries after, in nearly similar words, declared that they are in the organs by which many of their functions are performed, truly and strictly "inverted animals." Efforts, we have seen, are continually made in each successive stage to separate the vital functions, and concentrate their force in peculiar organs, for the better performance of their several duties: just as in mechanics, a pin is best made when the head is the work of one artist; the shaft of a second; the junction of each to the other the employment of a separate artificer; and sharpening the point the labour of an especial hand.

In reference to the proposition with which we set out, if we have found certain locomotive vegetables, as the Conferva Ægagropila, Vagabunda, &c., that can figuratively be said to have run away; these vagrant plants have run away without a stomach: and if Nepenthes and Sarracennia can, in anywise, be said to have "found out the blessing of a stomach," they have not been able "to run away with it."

Yours, very truly,

GILBERT T. BURNETT.

November 10th, 1829,
22, *Great Marylebone-street.*

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Streets of the Metropolis.

No person who has occasion to walk, or even ride along the streets of the metropolis, but has cause to complain of their bad, uneven, and often dangerous state. From the necessary excavations for water and gas pipes; for drains, ovens and cellars under the pavement; and for occasional repairs of the pavement itself, the streets, especially the leading ones, are ever in a state of disorder and confusion.

True it is, that such inconveniences must be submitted to in such a place as London. The alterations and improvements, both public and private, constantly going on, must necessarily keep the streets in a state of revolution. But why such works are not done in the best manner; and why the City has not the best engineers, as well as the best of everything else, is most unaccountable. In many cases, it is often observed, that improvements are begun before the projector is aware of the extent he may find it necessary to go, or before the mason or pavior is prepared with materials to complete the work. This is entirely for want of concert among those concerned in the affair, and for want, it would appear, of a competent superintendent to direct the operations. The greater part of this endless doing and undoing is caused by the imperfect and slovenly manner in which the excavations are filled up. The workmen's endeavour invariably seems to be, to make all good as expeditiously as possible; using any kind of material nearest at hand, whether it be fit or unfit; finishing by relaying the stone; and ramming the surface with ludicrous exertion of lungs and arms, till all is smooth and level. But in a short week or two, behold, the regularly paved place, instead of remaining what the workmen had called "a good job," is become a dangerous foot, or wheel trap; and then it must be pulled up again, to the annoyance of passengers, and at a further expense to the public!

To this improper and careless way of filling up these openings, is to be attributed the dangerous settling of the houses towards each other in narrow streets, and requiring those frightful shores to keep the walls upright. The foundations

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are disturbed by these repeatedly-opened trenches; and the heavy drays and waggons acting as pressers, shake the buildings from their position.

To avoid such danger and damage, and that the pavement should remain as durable and firmly level as the art and skill of the pavior can make them, it is, above all things, necessary that more labour be bestowed in filling up these openings. As no wise architect would build on loose ground, so neither should pavement of any kind, nor even a common road, be laid on a *substratum*, which is not sufficiently consolidated to bear whatever weights may have to pass thereon. To obtain this requisite compactness, it is necessary that the material which is dug out should be used or rejected, according as it is or is not capable of being rammed as firmly as possible. In doing this, the temperament of the material, whether sand, gravel, or other substantial earth, should be particularly attended to; it should be neither too wet, nor too dry; if just damp enough to knead together under the action of the rammer, it is in a proper state. Every shovelful thrown in should be subjected to the rammer, from the very bottom to the top; not a crevice or corner of the mass should escape. This operation carefully executed, as it may be, will ever after bear any kind of paving, gravel, or road metal, without sinking more than the surrounding natural bed of the earth.

An approach to this rule of management, and to prevent the irregular sinking of the pavior's work, has been done in Fleet-street; by placing a thick layer of broken stone below the pitching. This will certainly be effectual, where the stones are laid upon a firm bottom; but where laid on, or mixed with loose, unrammed earth, even this expensive expedient will be found insufficient.

In some other leading streets, where the Macadamizing system has not been introduced, the paving has been done with great exactness; regularly cut, nicely squared stones only have been used; and these, as usual, very smoothly rammed, and afterwards grouted with thin mortar to fill the interstices, and cement the whole together. But in such cases, the executor will be disappointed; he trusts too much to the accuracy of the workmanship in placing the stone, and to his powerful

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rank of rammers behind. The fact is, the rammers should be *before*, and not behind the men who place the stone. Pavement laid on a sufficiently solid foundation, needs very little after-ramming, if properly bedded in sand, and laterally connected. The labourer, in using his massive implement, directs all his attention and powers to produce *regular smoothness*, instead of *durable firmness*. The stone which happens to lie too high, receives repeated blows of the rammer, while the very next, perhaps, which has been placed too deep, is scarcely touched. This, it is true, makes neat, but not durably firm work; for unless every individual stone in the surface receives *an equal share of the rammer's force*, the pavement will soon become irregular from the action of heavy carriages.

To obtain, therefore, a smooth and durably firm pavement, the bed of soil on which it is to be laid should first be put into proper form, and equally consolidated; and, if possible, of equal consistence throughout. A carriage road, whether paved with stones, or Macadamized, requires a solid, yet somewhat *depressible bed* to rest on: otherwise the surface material would be soon worn down by the grinding action of wheels. The surface material, which yields a little to pressure, lasts much longer than a road which reposes even on a solid rock.

A few words may be added respecting the watering of the streets; this, when done timeously and properly, has many advantages sufficiently obvious. But it is too frequently done carelessly: too much water is thrown on at once, which not only lays the dust, but is soon trodden into a deep covering of filthy mud: the watering carts then disappear till this mud is again dry, and flying in clouds through the streets, when it is again diluted into mud. This watering should be done properly, or not at all; and were the dust, when moderately moist, swept off and carted away immediately, it would save the scavenger much trouble and expense, and render unnecessary their laborious and bespattering scoop-work. M.

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Analysis of a New Mineral, containing a hitherto unknown Earth, by Jacob Berzelius.

The mineral, of which the analysis is contained in the following paper, is found in syenite, upon the island Lov-on, lying off the coast of Brevig, in Norway. It was there discovered by Pastor Esmarck (son of Jens Esmarck, Professor of Mineralogy in the University of Xiania), who sent me a specimen of it for analysis, imagining, from its high specific gravity, that it might contain tantalum.

The mineral is black, without any signs of crystalline form or texture, and resembles entirely in external characters the gadolinite of Ytterby. Its surface is sometimes covered with a thin layer of a dark rust colour. It is very brittle, and full of cracks, on opening which the surface has a dull greasy lustre. On an entirely recent surface the fracture is glassy. It is heavy, having a specific gravity of 4.63. It is not very hard, being easily scratched by the knife. Its streak is greyish red. In powder, the mineral has a pale brownish red colour, becoming lighter the finer it is rubbed. Heated before the blowpipe, it loses its black colour, gives out water, and becomes nearly of the same colour as when reduced to powder, but does not melt. Heated to redness in an open tube, it gives off a very minute trace of fluoric acid.

With borax it melts easily, and if the mineral be in large quantity becomes opaque on cooling. No opacity is produced by *flaming*. The colour of the glass indicates the presence of iron. With saltpetre it shews also the presence of manganese. With phosphoric salt it melts, leaving a residue of silica, and the glass, which is coloured by iron, becomes opalescent on cooling. Here also, with saltpetre, the presence of manganese is indicated. With carbonate of soda the mineral is decomposed without melting, and leaves on the charcoal a yellow brown slag. By *reduction* *, if borax be added, small white metallic globules are obtained, which flatten under the hammer. They are lead, containing a trace of tin. On a plate of platinum, with carbonate of soda, the mass becomes green.

* In the inner flame of the lamp.

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The mineral seems to occur very sparingly. Professor Esmarck has informed me more lately that no new quantity has been obtained, as the closeness of the locality to the surface of the water prevents the rock from being blasted until the water be frozen.

The mineral contains a metallic body hitherto unknown, which, in its properties, belongs to the class which forms the so called *proper earths*, and its oxide is an earth which most nearly resembles zirconia, and, what is singular enough, possesses the greater part of the properties and characters found in the earth I formerly described, under the name of Thorina. This circumstance led me at first to imagine that thorina might possibly not have been, as my later experiments seemed to shew, simply a sub-phosphate of yttria, but a mixture of it with thorina. At the beginning of this investigation, therefore, I gave the name of thorina to the new earth,—and when afterwards, by a new analysis of what still remained of the mineral, in which I supposed myself to have found the *older* thorina *, I could not detect a single trace of the *new*, I thought I could, with still greater propriety, retain for this last the same name, both because the former description for the most part agrees with it, and because the name has already been once introduced into the science. The new mineral itself I have consequently called thorite †.

ANALYSIS OF THORITE.

a. 2.005 grammes in coarse powder were put into a small tube retort, connected by caoutchouc with a receiver, from which the gas given off was conducted through a tube filled with chloride of calcium. The loss, by heating to redness, was 0.1985. Of this, 0.19 was taken up by the receiver and the chloride of calcium, and consisted of water which gave a minute trace of fluoric acid. 0.0085 was gas given off.

The same portion, again heated to redness in a stream of

* It appeared to me probable, that eudialyte from Greenland might contain thorina, since, at the time when Stromeyer analyzed that mineral, the properties of zirconia were not so well known as now, and therefore the new earth might be mistaken for zirconia; but I found in it, as Stromeyer has stated, only zirconia.

† Professor Esmarck calls it berzelite.—Tr.

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hydrogen gas, from reddish brown changed to a blue grey, inclining to green, and lost again by the formation of water .03. Rubbed in the mortar, the mineral now gave a dark grey powder, very little acted upon by muriatic acid.

b. Five grammes of thorite unheated and reduced to fine powder, being treated with muriatic acid, became yellow, and gave a slight odour of chlorine. By heat the evolution of chlorine became stronger, and the whole was gelatinized. Evaporated in a water-bath it left, after solution, .985 grammes of silica. This was dissolved afterwards by boiling in carbonate of soda; the solution diluted with boiling water, the clear liquor decanted, and the remainder boiled over again with carbonate of soda, there remained undissolved in the alkali small grains of quartz, a little powder of the stone which had escaped solution, and a light grey yellow powder which, by decantation, could be separated from the foregoing. This powder weighed .05 grammes; the heavier weighed .018, or together .07 grammes; after which there remained of undissolved pure silica .915 grammes. The grey yellow powder contained much silica in its composition, and, with carbonate of soda, before the blowpipe melted into a glass. I have not examined this more minutely.

The aqueous solution which had been separated from the silica, was precipitated with caustic ammonia, and the precipitate well washed with boiling water. The ammoniacal liquor which passed through was mixed with the evaporated washings, saturated with oxalic acid, and slightly heated, till the precipitate, which immediately appeared, had completely fallen. The precipitated oxalate of lime, burnt and treated with carbonate of ammonia, gave a slightly brownish carbonate of lime, weighing .241 gr. It was dissolved in muriatic acid, and the solution mixed, first with aqueous solution of bromine, and afterwards, in a corked flask, with very dilute caustic ammonia, till the acid was, in a minute degree, oversaturated. After twenty-four hours, there had separated from the solution, which, by degrees, became yellow, a quantity of oxide of manganese, which, heated to redness, weighed .01 gr. The true weight of the carbonate of lime, therefore, was .23 gr., answering to .1288, or 2.576 per cent. of pure lime.

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c. The solution, remaining after precipitation by oxalic acid, was evaporated to dryness, and the sal ammoniac driven off by heat. The remainder washed with water left .018 grammes of magnesia, slightly tinged by oxide of manganese, which was in too small quantity, however, to admit of separation.

d. The washings by evaporation gave .0205 gr. of mixed chlorides of potassium and sodium. These were separated by chloride of platinum — the salts after mixture with it being dried, and the soda salt separated by alcohol. In this way the salt was found to contain .0113 chloride of potassium, and .0092 chloride of sodium, answering, the former to .007 gr. of potash, and the latter to .0049 gr. of soda.

e. The precipitate (from *b*) became dark by washing from the presence of protoxide of manganese. It was dissolved again, while still moist, in muriatic acid, and the filter carefully washed. Through the solution a stream of sulphuretted hydrogen gas was passed, which separated a black precipitate. From this precipitate well washed, hydro-sulphuret of ammonia separated a minute trace of sulphuret of tin, too small to be collected or weighed. The precipitate was treated with nitric acid to full oxidation—afterwards a little sulphurous acid was added, and the whole evaporated by a gentle heat till the excess of sulphuric acid was driven off. From the dry mass water separated a metallic salt, from which ammonia gave a precipitate in white flakes, which weighed .005 gr., and which before the blowpipe exhibited all the properties of the oxide of tin, and was reduced with carbonate of soda to white malleable globules. That which remained insoluble in water was sulphate of lead, and weighed .052 gr., answering to .04, or .8 per cent. of oxide of lead.

f. The liquid, after precipitation by sulphuretted hydrogen, was evaporated to dryness by a gentle heat. Towards the end it gelatinized, and left, after solution in water, .034 of silica. The solution was precipitated by caustic potash, which was added in excess, and the precipitate boiled with it. The alkali dissolved .003, which, at a red heat with *cobalt solution*, became blue without melting, and was, therefore, alumina. Neither this nor the alkaline solution contained any trace of phosphoric acid.

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g. The precipitate thus treated with potash dissolved with ease in diluted muriatic acid, leaving peroxide of manganese, which, washed and heated to redness, weighed .081. There was found, also, a trace of oxide of iron, and of thorina, but too small to admit of separation.

h. The solution in muriatic acid was neutralized by caustic ammonia, and concentrated by evaporation, after which pure sulphate of potash was dissolved in it to complete saturation. There fell a fine white powdery precipitate, which was thrown upon the filter, and washed with a saturated solution of sulphate of potash. It was then dissolved from the filter by boiling water, which took it up without residue. The solution, precipitated by caustic potash, gave a white earth, which did not become yellow by washing, by which the absence of cerium was shown, and which, heated to redness, weighed 2.817 grammes, and was thorina, with a slight, yet low, tinge, from a trace of oxide of manganese, which could not be quantitively separated, but the presence of which was also shown by carbonate of soda on a slip of platinum. Its quantity is, at all events, too small to affect the weight of the earth, in any sensible degree. In further trials, made upon this earth, it was found free from all other mixture.

i. The solution, precipitated by sulphate of potash, was again precipitated by caustic potash, the precipitate well washed, and treated afterwards with carbonate of ammonia. The portion insoluble in carbonate of ammonia, heated to redness, weighed .1905. It was dissolved in muriatic acid, and decomposed, in the usual way, by succinate of ammonia into .162 peroxide of iron, and .0285 peroxide of manganese.

k. The solution in carbonate of ammonia was evaporated to dryness. The dry mass was digested with dilute acetic acid, which became of a yellow colour, and with caustic ammonia gave a beautiful bright yellow precipitate, which, after washing and heating to redness, became dark green, and weighed .079, proving to be protoxide of uranium.

l. The residue, after digestion with acetic acid, was yellow grey with muriatic acid: it gave a colourless solution, which was first mixed with tartaric acid, and afterwards supersaturated with ammonia, without the appearance of any preci-

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pitate. Sulphuretted hydrogen separated a minute portion of sulphuret of iron, which, dissolved in nitric acid, and precipitated by ammonia, gave .008 peroxide of iron.

m. The liquid precipitated by sulphuretted hydrogen was evaporated to dryness in a weighed platinum crucible, the sal ammoniac driven off, and the tartaric acid destroyed by burning, after which there remained .073 of a light yellowish earth, which was found to contain neither yttria nor titanic acid, and which, in all respects, comported itself like thorina, tinged with peroxide of manganese. I ought on this occasion to mention, that the presence of thorina in the solution after precipitation by sulphate of potash, is an error in the process, arising from the circumstance, that the earth was not fully precipitated by the sulphate of potash; an operation, however, which is very easy when the solution employed is not too concentrated. I shall return to this subject in describing its double salt.

The whole results of the analysis give for the composition of thorite :—

	in 5 grammes.	in 100.
Thorina *h*) 2.8175 + *k*) 0.079 . . .	= 2.8905	= 57.91
Lime *b*)	0.1288	= 2.58
Oxide of iron *i*) .162 + *l*) .008 . .	= 0.1700	= 3.40
Oxide of Mang. *b*) .01 + *g*) .081 + *i*) .0285 .	= 0.1195	= 2.39
Magnesia *c*)	0.0180	= 0.36
Oxide of uranium *k*) .079 oxidule + .014 oxygen	= 0.0804	= 1.61
.. of lead *e*)	0.0400	= 0.80
.. of tin *e*)	0.0050	= 0.01
Silica *b*) 0.915 + *f*) 0.034 . . .	0.9490	= 18.98
Water *a*) $\frac{0.19 \times 5}{2}$	0.4750	= 9.50
Potash *e*)	0.0070	= 0.14
Soda *e*)	0.0049	= 0.10
Alumina *f*)	0.0030	= 0.06
Undissolved mineral *b*)	0.0700	= 1.70
Loss	0.0359	= 0.49
	5.	100.

Since, in this analysis, chlorine is developed during the solution of the stone, it is plain that both the iron and the manganese exist in it in the state of peroxide. From the investigation into the saturating power of thorina, hereafter detailed, it follows, that the oxygen, in all the bases together, is equal to

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that contained by the silica. That in the thorina is a little more than twice the oxygen in the other bases; but both the great number of the latter, and the mixture of bases having one, with others having three atoms oxygen, between which no multiple ratio can obtain, induce me to consider thorite as an accidental mixture of several hydrated silicates.

(To be continued.)

Notice on the Observations of the Comet of Encke during its appearance in 1828. By M. Struve.

[From Le Recueil des Actes de la Séance Publique de l'Académie Impériale des Sciences de St. Pétersbourg, 29 Dec. 1828.]

At the annual meeting of the Imperial Academy of Sciences at St. Petersburgh, December 29, 1828, a notice was communicated by M. Struve, on the Observations of the Comet of Encke, made in the observatory at Dorpat, in the autumn of 1828, with the great refracting telescope of that observatory.

The comet was first seen by M. Struve on the 16th of September, New Style, which was several days earlier than it was seen elsewhere. It then appeared as an extremely faint nebulous spot; but early in October, and at a time when all other observers complain, that, from its faintness, they were unable to make satisfactory observations, the superior light of the Dorpat telescope enabled M. Struve to commence a series of exact determinations of its place. The micrometer attached to the telescope,—the spider-threads of which are illuminated, whilst the field remains dark,—is also peculiarly suited to the observation of very faint objects.

M. Struve's series of observations commence on the 13th of October, and terminate on the 26th of December, comprising an interval of nearly two months and a half. By submitting the observations to the calculation of probabilities, he finds the probable error in the determinations of right ascension and declination, between the 26th of October and 10th of November, to be about two seconds of arc, and from the 30th of November to the 25th of December, to be less than one second.

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An highly interesting and curious part of M. Struve's communication is the remarks with which it concludes on the inferences which may be drawn from the observations in regard to the physical nature of the comet. We shall not attempt to shorten these remarks, lest we should do them injustice.

I. When first seen, the comet had a diameter of between two and three minutes; yet, from its extreme faintness, was scarcely visible. Now, as objects shining by their own light become invisible, not from increasing faintness, but from reduction of magnitude, it follows, that Encke's comet is probably a body shining only by the reflected light of the sun.

II. On the 29th of October, the comet covered a star of the 9—10th magnitude, which passed within 22 seconds of the most luminous (and therefore the densest) part of the comet, without losing any portion of its brilliancy. On the 7th of November, a star was seen so situated as to be at first mistaken for a nucleus shining through the nebulous matter of the comet, until its motion discovered its real nature. And on the same night another star was covered by the comet, and passed within a very few seconds of its most luminous part, without being in the least degree obscured. From these observations, it appears that the comet had no solid [opaque] nucleus.

III. In regard to the form and appearance of the comet, as early as the 7th of November, a nebulous spot, more luminous than the rest, could be distinguished, extending, as shewn by the plate, from *a* to K. (*Vide* Lithographic Plate, Fig. I. and II.) This spot was not simply a condensation of the nebulous matter towards *k*, the most luminous point, for it had a well-defined limit at *a*, whilst at K the limit was not so well defined as at other parts. On the 30th of November, the limit of the spot and the exterior limit of the comet coincided at *a*, and at this time there appeared to be at *k* a nucleus, with imperfectly defined limits, shining through the nebulous matter in which it was enveloped. It was this point *k*, the centre of the apparent nucleus, which was adopted throughout

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as the precise point to be observed in determining the place of the comet. The figures in the accompanying wood-cuts shew

Fig. III. 7th Dec.

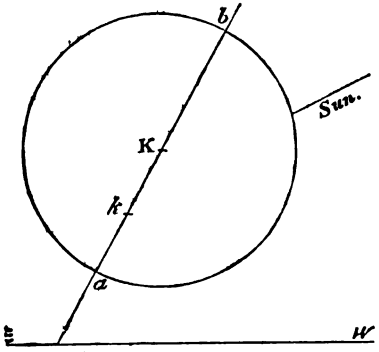
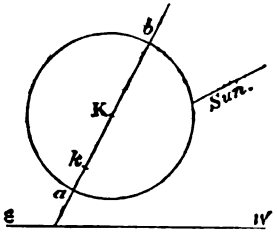


Fig. IV. 14th Dec.



the eccentricity of this point, as seen on the 7th and 14th of December. These two figures, from the first having been drawn under unfavourable circumstances, and the latter whilst the moon was shining, do not quite accord in shape with the correct representations in the lithographic plate.

IV. On the 7th of November, the whole nebulous mass had a diameter of eighteen minutes, and on the 30th of November it had lessened to nine minutes, although, during the interval, the comet had approached nearer to the earth, and although, from its increased vicinity to the sun, its light should have been twice as strong as on the 7th of November, and consequently its extent and limits more clearly visible. The fact of the contraction of the whole nebulous mass was further confirmed by the coincidence, on the 30th of November, of the limit of the whole mass with the boundary of the more luminous included spot; whereas, on the 7th of November, the boundary of the spot was considerably withinside that of the nebulous envelope. M. Struve is positive in the recollection of his surprise at the diminished size of the comet on the 30th of November, at which time he had expected to have found it much larger than on the 7th; particularly as, from its increased light, it had become visible to the naked eye as a star of the 6th magnitude.

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V. If the line *a b*, which in all the figures passes through *k*, the most luminous point, and K the geometric centre of gravity of the whole figure, be called the axis of the comet, all that part which is on the side of *b* may be regarded as a tail occupying both sides of the axis. The best defined boundary was always on the side of *a*, and the least defined, on the side of *b*; the former taking a parabolic curve from *a* towards *c* and *d*. But the point most deserving of notice, is the direction of the axis in respect to the sun. Its apparent direction was determined four times on the 7th and 14th of December; and the apparent direction of the tail of an ordinary comet in opposition to the sun being calculated, differences amounting to 100°, 145°, 154°, and 149°, appeared between the observed and the calculated directions. Thus the direction of the diffused or spreading part of the comet was always turned rather towards than from the sun, offering, in this respect, an absolutely new phenomenon; the more remarkable, because the best defined part of the contour, both of the envelope and of the nucleus, were on the side from the sun. If we compare these observations with those which have been made on other comets, Encke's comet will appear to have something in its nature different from other comets, and to be, in some respects, opposed to them; insomuch that we might be disposed to name it *a negative comet.*

VI. The appearance of this comet, when seen in 1825, was, to the best of M. Struve's recollection, that of an almost planetary disc of uniform light. If this recollection, grounded on observations made with a 5 feet telescope of Troughton's, can with propriety be compared with the observations of 1828, the appearance offered by the comet on its last return, was essentially different from that which it presented on the former occasion; and if so, we may anticipate important results from future observations.

"Will," M. Struve concludes, "the direction of the tail of this comet towards the sun, and the diminution of its magnitude, on approaching that powerful luminary, justify a supposition, that the sun attracts to itself a portion of the mass of this comet, which being thus subjected to successive diminutions, will finally be absorbed altogether, and the comet cease

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to exist in a separate form? Or may the diminution in its size, as well as its different appearance in 1825 and 1828, be attributed to a concentration of the matter of which the comet is composed?

"The solution of these questions must be left to future observations."

Observations on Quinia, or Quinine, and the preparations of Cinchona, by J. Hancock, M. D.

Quinine is now almost universally commended, both in and out of the profession.—Is this to be considered an absolute proof of its transcendent excellence, or can it be accounted for on other principles?—We know, that amongst the Chinese the root ginseng, considered amongst us as a very simple and innocent drug, is equally or still more extravagantly esteemed than quinine is at this moment with us.—There is a fashion in physic, as in everything else, and that a very changeable one in respect to remedies.

Pharmaceutical, like other hypotheses deduced from false premises, are the sources of innumerable errors. Seguin, an eminent French chemist, finding that infusion of bark was precipitated by galls, concluded, that it could be no other than gelatine which constitutes the febrifuge principle of the bark. Whilst this absurd error remained undetected, i. e. of the bark containing gelatine, common glue was found so successful in the cure of intermittents, as scarcely ever to fail or to disappoint the practitioner!

On this subject, we are greatly indebted to Dr. Paris for his firmness; for he appears to be the only author who shews the least disposition to oppose the unbounded innovations of the pharmaceutical chemists of the day. "It is only necessary in this place," says he, in vol. ii. p. 165, "to caution the practitioner against the too hasty generalizations of the too sanguine chemist; it has already been observed that those vegetable remedies, whose value has been established by the sober experience of ages, consist of different principles of activity, or, at least, owe a modified power to the compound of their several

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ingredients." And in vol. i. p. 285, he adds, "Does it not, therefore, appear that certain elements exist in the composition of vegetable remedies, as furnished by nature, which, although *individually* inert, confer additional strength and impulse upon the principle of activity with which they are associated *?"

So far, indeed, from depriving the bark of a large portion of its active principle, the most sensible physicians have advised the addition of other aromatic and astringent substances to heighten its powers. In proof of this, I shall quote further from Dr. Paris's work.

The most powerful minds are not exempt from extravagant errors.—Sydenham pronounced that "to add anything to the bark argues either ignorance or craft." He did not, however, intend at the same time that anything should be abstracted from it.

"The most respectable testimony," says Dr. Paris, vol. i. p. 307, "may be adduced to demonstrate the great advantages which have arisen from the various combinations of this heroic remedy."—Sir George Baker has said, that "there is less of reason than of severity" in the above remark of Sydenham; for that it was found in the cure of the intermittent which he describes, that, according to circumstances, sometimes the Virginian snake-root, and in other cases myrrh, were added with propriety and advantage; and, according to the experience of several practitioners, a drachm of the rust of iron, and the same quantity of black pepper, added to each ounce of the bark, were the means of subduing the most inveterate agues.—

* When the structure of vegetable remedies shall have been thoroughly examined upon this principle of combination, much medicinal obscurity will be removed, and probably pharmaceutical improvements of value suggested; at all events, it will teach a lesson of prudent caution to the pharmaceutic chemist; it will shew the danger of his removing this or that element from a vegetable compound, merely because he finds, upon its separation, that it is inert. I dwell the more upon this point, because I feel that there never was a period in the history of medicine, at which such a caution was more necessary; for while the poly-pharmacy of our ancestors has driven the physician of the present day into a simplicity of prescription that on many occasions abridges his powers and resources, the progress of chemical knowledge has diffused through the class of manufacturing chemists a bold spirit of adventure and empiricism,—a mischievous propensity to torture our best remedies, in order to concentrate or extract the parts which they consider to constitute their essential ingredients."

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Hillary speaks of an epidemic intermittent at Barbadoes, in which "the bark was of no avail, unless combined with saline remedies, or some of the tonic bitters."

In a subsequent page, Dr. Paris adds, "I shall conclude the present section by shewing, that many of our most valuable vegetables owe their useful properties to the joint operation of the several *distinct* and different ingredients which enter into their composition. How many substances does nature produce in the vegetable kingdom, in which the permanent tonic quality of bitterness exerts its influence in union with the transient stimulating powers of an aromatic principle? *Peruvian bark* may be said to combine within itself the properties of *bitterness*, *astringency*, and *aroma*; a fact which suggested the probability of our being able to produce an artificial compound that might emulate the effects of cinchona, and to a certain extent the idea appears to have been realized; for we are told by Dr. Cullen, that he frequently succeeded in the cure of an intermittent by a combination of *oak bark* and *gentian*, which neither bitters nor astringent, separately, produced the least impression; and I am informed by Dr. Harrison, that in the Horncastle Dispensary, of which he was for many years physician, he never employed any other remedy for curing the ague of Lincolnshire than equal parts of *Bistorta* (astringent) and *Calamus aromaticus* (bitter and aromatic), neither of which plants, *individually*, ever produced the least benefit in such diseases. Berzelius attempted to produce a compound of this description, by adding to the bark of the *ash* some *tormentil root* and *ginger*; and he observes, that it acted as an excellent tonic, and that, according to the experiments of his friends, it seemed to cure quartan agues*. In the aromatic barks and woods, such as those of the *canella*, *orange peel*, *sassafras*, &c. the aromatic principle† is combined with a bitter ingredient; a union which proves of singular service

* "Dr. Young's Medical Literature," 2d edit. p. 570.

† "The vegetable kingdom presents us with many natural compounds of this kind; several of which might be pressed into the service of medicine with much advantage. With respect to the number and variety of such substances, it must be confessed that our Pharmacopœia contains but a meagre bill of fare."

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in the formidable bowel complaints so common in tropical climates."

Here is a weight of evidence added to our *daily experience* in proof of the efficacy of astringents and aromatics, both as febrifuges and as general tonics.

Now it is vulgarly believed, and indeed we frequently hear it asserted by those who ought to know better, that the sulphate of quinine contains all the virtues of the bark. A very little reflection, however, must convince any unbiassed mind of the falsity of this opinion. It neither contains the astringent nor the aromatic principle; and a large portion of bitter extractive is also rejected with the infusion, in the process for the manufacture of sulphate of quinine. All these are essential to the entire powers of the bark, as we know from their tonic effect on the system. The *resino-extractive* matter was considered by Murray as the principle of the greatest activity of any the bark contains. " Astringents," says Dr. Paris, vol. i. p. 152, " are capable of acting as permanent stimulants, of curing intermitting fever, and of obviating states of general debility.' These principles are entirely thrown away in the process—and as to the invaluable antiseptic properties of the bark, the quinine contains nothing of it.

All this is evident from the methods taken for the preparation of quinine. Of those persons who assert, that the quinine contains all the virtues of the bark, very few, I believe, can have reflected upon the process employed in its preparation. The following one, from the Paris Pharmacopœia, will serve as an illustration. A tincture of the bark is first made with alcohol, which is evaporated to an extract. This extract is next washed with repeated portions of watery solution of potass, till it ceases to colour the water, and then with pure water to wash away the potass. This residuum then, divested of everything soluble in water, together with the agency of potass, is the meagre portion of the bark which remains, and which is now to be digested in an acidulated liquor (*acide hydrochlorique*). This liquor is next to be filtered, boiled on magnesia, filtered again, and evaporated to dryness. This residue is ultimately to be digested in alcohol, filtered, and the tincture evaporated,

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which, after this long and tedious operation, leaves the quinine, not much improved, I presume, in the essential properties of the bark.* (See the Pharm. Univ. 1828, tom. ii. p. 374.)

It is well known, on the other hand, that the simple infusion of the bark, taken freely, will cure intermittent fevers, although it is supposed to contain little or no quina or quinine, for without the acid it is considered nearly insoluble in watery fluids. This doubtless is the case; for I find that the *sulphate* of *quinine* is scarcely soluble in water alone, and, even in hot water, it is very imperfectly so; but on adding sulphuric acid, it dissolves instantly, like ice thrown into hot water. In spirituous menstrua, it is also very readily soluble.

All the world, however, agrees, that quinine is an absolute specific for intermittent fever: according to the old adage, "Every body says so, and it must be true!" This I consider a mere hue and cry, emanating from high authorities, founded rather on the estimation in which the Peruvian bark itself is justly held, than on patient investigation and careful comparison of results. The simple infusion of quassia likewise cures intermittents, especially if taken warm.

That those alkaloids, the quinine, cinchonine, &c. do contain, in a certain degree, the febrifuge properties of the bark, I by no means intend to dispute; and therefore, in so far as this fact is well established, let us freely offer our meed of praise to those distinguished continental chemists for their valuable dis-

* We know that alcohol is the most effective solvent for nearly all of the active vegetable principles—or those capable of acting with energy on the animal economy,—(gum, albumen, &c. pertaining exclusively to a watery menstruum, are comparatively inert)—yet the *active principles* are, in different degrees, soluble in water also;—the substances acted on must, therefore, after repeated coctions, washings, exsiccations, &c. retain but very little of those constituents on which their natural and clinical powers depend—especially in a substance like the Cort. Cinchonæ, consisting of *several distinct elements*, all of which are more or less necessary to its full and proper action, and which, it is quite evident, are thrown away in the process for obtaining quinine.

† It is not to the French chemists, however, but to Dr. Duncan, of Edinburgh, I presume, that we owe the establishment of that peculiar principle or agent which is considered the more active one, in the bark of cinchona, a circumstance which seems now to be, in a great measure, overlooked, and for the reason, I suppose, that the same learned physician had not indulged in the affectation and quackery of applying every discovery or new principle, right or wrong, to some use as a medicinal agent!

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covery, (*valeat quantum valere debet*), less, however, for procuring the virtues of the bark in a minute form, than for promulgating a means of eliciting an active principle, most easily available in aqueous fluids, which may be considered a very important point †. Experience and reflection, however, have abundantly taught me, that these properties are most extravagantly exaggerated.

I could adduce many instances in support of this fact, but I shall allude only to one or two here.

Mr. Chandler, of Essequibo, had fever whilst in George Town (March, 1827). It continued for seven days. On the intermission, (having been duly evacuated,) he began taking quinine in the dose of 3 grs. every hour. The fever returned with severity, after an intermission of about twelve hours, when he had taken not less than 35 grs. of quinine. This paroxysm lasted for six or eight hours. On the next intermission, he commenced the remedy, and took to the extent of about 40 grs. The fever again returned, with little less violence than before. Thus we may say he had one return after having taken 35 grs., and another after having taken 70 grs. This would cost, at the druggists in Demerara, about 24*s.*, equal to the expense of two pounds of the best bark, which would be sufficient to arrest the progress of a dozen regular intermittents! But it should be observed, that a fever of the continued form, as in this case, seldom returns, although no bark or other remedy should be employed as a preventive.

A year ago I had a fever, in all respects similar to the case here noticed: it continued seven or eight days. I took no quinine or bark in any form; and I had no return of fever. Yet, Mr. Chandler, though a gentleman of sterling good sense, attributed his recovery entirely to the quinine. This, however, only shews how men of the soundest judgment are liable to be imposed upon.

A neighbour, Mr. M'Lean, entering the room at the time I was making some notes on this affair, began relating to me his own case. Before having heard my opinion, he said, that in the fever he had lately passed through, he had tried the quinine largely, but got no relief from it; that he was ultimately obliged to have recourse to the bark powder; that he had no re-

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turn of fever after commencing with this, and that he found his appetite wonderfully recovered, and his strength increased by it.

The quinine employed, in both those cases, was known to be genuine, from the great manufactory at Paris, and was received through one of the most respectable houses in London.

Besides the absurdities above alluded to, there are various others creeping in, and those, too, sanctioned by authority, or at least by modern writers on medicine and pharmacy.

A new formula for quinine has lately become fashionable, which serves to exemplify the rage for new remedies, however absurd they may be, if introduced under approved auspices. It consists of 4 or 6 grs. of quinine to an ounce of alcohol, so that to take a dose of quinine, it is necessary to swallow what is equivalent to a wine-glassful of proof spirit. In Rennie's Supplement to the "Pharmacopœias," (page 451,) 4 grs., which may be esteemed but a common dose, as now employed, are directed to the ounce of alcohol of specific gravity .847, although a few drops would answer the purpose of dissolving the quinine. This is probably adopted from the French codex, and certainly the *march of intellect* is rendered evident, by its having found its way so early into our dispensatories. It will be a great comfort and convenience to be able to get a good strong dram under the delicate name of *Tincture of Quinine.* We shall see if the college of physicians will adopt this valuable recipe in the next edition of their "Pharmacopœia."

Another of these prescriptions is, to dissolve 12 grs. of quinia in two pints of madeira or sherry. This, I suppose, may be that which is puffed off, under the name of the Tonic French Wine (see Rennie, p. 419) for training!

These new remedies having been got up, without the imputation of *empiricism*, and ostensibly as the pure results of pharmaceutic or chemical research, is the reason they have been so favourably regarded by the members of the medical faculty, who, indeed, have received them as a boon almost without examination. Some few of the profession, however, we find have paused, or have dared to make a stand, and allow the tide of popular clamour on the infallibility of quinine

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to roll past them. The judicious Dr. Paris, as already observed, is the foremost one of the very few who have evinced sufficient strength of mind to resist the overwhelming current.

It should be observed, too, that *alkalies*, or analogous substances,—as potass, ammonia, magnesia, lime, barytes, &c. are invariably directed to be employed, as essential in the preparation of our *new alkaloid* remedies; and how is it possible not to suppose that the alkalines employed must, in some measure, modify the results?

Piperine, procured after a similar manner from black-pepper, said to be febrifuge and poisonous in large doses, is regarded as possessing the essential properties of black-pepper. Common sense, however, if it had confidence to hold up its head in the presence of *affected chemical science*, would reject such absurd opinions,—as the said piperine is, as we are told, an *insipid* powder, and totally destitute of all the sensible properties of that most potent and pungent aromatic berry of which it is the pretended essence!

And with respect to *emetine*, it is now well known, as asserted by Mr. Brande five or six years ago (Man. Pharm.), that "it cannot be safely depended on as a substitute for ipecacuanha. Although, therefore, with a heap of other novelties, it has a place in the Paris Pharmacopœia, it seems very doubtful whether it will ever be much used in British practice."

There appears, moreover, to be less economy in the preparation of quinine than was at first announced. It was stated that 8 or 10 grs. would generally arrest an intermittent; but it has now got up to 100 grs. or more: to procure which, nearly a pound of bark is required.

It may be observed, too, that the new principle called kinic acid, or kinate of lime, is also disregarded and thrown away in the process of forming the quinine or cinchonine. This substance appears to possess much of the sensible properties of the bark. It is extracted by macerating the bark in cold water, from which, by rest, the kinate of lime separates in plates. The lime being separated by oxalic acid, the kinic acid is left pure. It is of an acerb, bitter taste.

What has been said of quinine, is applicable to cinchonine. I have made no distinction, wishing, for brevity sake, to be

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understood as including both under the former title, because we have reason to consider them both merely as modifications of one and the same principle, procured by the same process, and possessing similar medicinal properties.

Although it is plain, however, that quinine is far from containing all the active elements of the bark, still the French discovery will, if the foregoing views are correct, contribute to indicate a means of obtaining all its virtues in a light and not unpleasant form, which has long been a great desideratum in pharmaceutic chemistry; and such a preparation, I presume, will be found in the infusion hereafter proposed, with addition of sulphuric acid, &c.

Preliminarily, I may observe, that Murray, in his "Materia Medica," has the following passage:—"In those varieties of continued fever, connected with debility, as in typhus, cynanche maligna, confluent small-pox, &c., Peruvian bark has been regarded as a valuable remedy. It is difficult, however, to give it in such quantities as to obtain much sensible effect from it, as, from the weakened state of the organs of digestion, *it remains on the stomach unaltered*, and is liable to produce nausea and irritation." This points out the necessity of searching for a method of extracting the active principles, (not a single insulated element,) so as to administer them free from the inert woody fibre.

In alluding, also, to the difficulty often experienced in giving the bark in substance, Dr. Duncan says, "An important objection is, that some stomachs will not bear it, from the oppression and even vomiting which it excites; that it has long been a pharmaceutical problem to discover what menstruum extracts cinchona most completely; and that we must have recourse to direct experiment to determine the degree of action possessed by each menstruum upon it. With this view many experiments have been made, and by very able chemists. But most of them were performed when the science of chemistry was but in its infancy; and even at this time, that branch of it which relates to those substances is so little understood, that the results of the latest experiments are far from conclusive*."

* "Edinburgh New Dispensatory," p. 113.

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Mr. Brande, in the "Manual of Pharmacy," even recommends the sulphate of quinine to be taken with diluted sulphuric acid, 15 minims, (nearly 20 drops to the dose,) with each grain or two of quinine. He observes as follows:—"Sulphate of quinine has been exhibited in doses of from 1 to 5 grs., three or four times a day, though 8 grs. are thought by some to be equivalent to an ounce of bark. It appears to me most prudent to begin with small doses; and, if they answer the intended purpose, not to increase them; if not, they may be augmented till the disease feels the influence of the remedy; but in large doses this salt will be found to nauseate, to create weight and pain in the stomach, attended by thirst, a white tongue, a quickened and hardened pulse*, symptoms which of course should be watched." This does not well correspond with the characters of the *bark* as an *antispasmodic*. Our author continues—"It has appeared to me to agree best when administered with dilute sulphuric acid, provided this plan is not otherwise contra-indicated. The following formula may therefore be adopted:—

℞ Quinæ Sulphatis ℈j;
Conservæ Rosæ, q. s. ut fiat pilulæ xx.
quarum sumatur una vel duæ ter die cum hausta sequenti.
℞ Decoct. Cinchonæ ℥j, Acid Sulph. dilut. minims xv.
Syrup. Aurant. ℥j misce."—p. 63.

Here we observe, that the acid is ordered to be added to the decoction already prepared, merely as a useful adjunct, and not with a view to evolve the quinine in the decoction, which could be done, only by putting them together at first, or digesting the acid upon the bark prior to straining. From this recipe, however, of Mr. Brande, we derive a strong testimony in support of the propriety and advantage of the method hereafter proposed; for if, as stated by this distinguished professor, the acid is to be deemed the best addition to the bark from its own intrinsic value, how much more estimable and important should it be considered, when applied in such manner as to elicit and bring into action that latent principle, the quinia, in which the chief energy of the bark, as a febrifuge, is supposed to reside!

* Its action will, doubtless, be somewhat varied in different constitutions. I have recently been told by a friend of mine, Mr. Edward Dawson, who has tried the quinine, both here and in Demarara, that its use has, with him, been invariably attended by headache and dizziness.

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Dr. A. T. Thomson, in the "London Dispensatory," p. 246, takes occasion to notice the efficacy of the bark in low typhoid continued fevers, in various malignant and putrid disorders, and in pure typhus; but he conceives—"that the best effects will be produced by the bark, when its use, in pure typhus, is delayed till the skin becomes moist, the tongue in part cleaned, and the urine deposits a critical sediment. The best adjuncts in these cases are the diluted sulphuric, or the muriatic acids, and aromatics, particularly the tincture of *capsicum*."

At page 737 of the same work, it is remarked of the infusion of the bark, that "the sulphuric acid destroys the bitterness of the infusion, but not its astringency; and adds considerably to its efficacy." Possibly it may destroy the bitterness of the infusion by being added at the end of the process, or it may only cover the taste and render it less perceptible; but when digested on the bark with hot water, if the acid be not in excess, or be added in sufficient quantity only to neutralize the alkaloid principle, it seems to me rather to augment than to diminish its bitterness.

It would even appear that a vague idea has long since been entertained regarding the utility of sulphuric acid as an adjunct to the preparations of bark.—"It (the bark) is used for obviating the dispositions to nervous and convulsive diseases; and some have great confidence in it, joined with the acid of vitriol, and notwithstanding the latter, a milk diet,—in cases of phthisis, scrophula, ill-conditioned ulcers, rickets, scurvy, and in states of convalescence."—*Lewis's Disp.*

"Il n'est pas rare que les fiévres intermittentes bilieuses du printemps et de l'été deviennent putrides; les malades tombent dans un grand affaissment, et sont couchés comme des automates; le pouls est à pein sensible; le sang tombe en dissolution:—sans le quinquina dans ces circonstance, la plupart mourroient. On le donne alors en decoction, plutot qu'en poudre, uni avec les purgatifs, et encore mieux avec l'acid vitriolique."—*Cours Elémentaire de Matière Medicale de M. Rochfort.* Paris, 1789.

With a view to the investigation of this important subject, I instituted some experiments in September, 1827. On the 7th,

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℥j of bark* was infused in 8 oz. of hot water, with ʒii of diluted sulphuric acid. This had scarcely any sourness, or not more than water with half the proportion of acid. The infusion had, however, a strong taste and odour of the genuine cinchona. In a subsequent experiment *not the smallest separation or change* of appearance was caused by adding the sulphuric acid to the infusion previously filtered, nor, on the following day, had the least change ensued.

The infusion of caramacata (a bitter febrifuge bark of the Orinoko) also takes off the acid taste of the sulphuric acid; but I found, on adding it to the settled infusion, that a copious yellow precipitate was presently formed.

This would, therefore, seem to warrant the conclusion, that whilst a certain principle in the Peruvian bark (probably cinchonia) which combines with the acid, remains soluble in water; a part, in the caramacata infusion, which combines with the acid, is insoluble in watery menstrua, and is precipitated.—This may be the active principle of the latter bark.

The cinchona infusion remained covered, and exhibited little or no change on the 20th. This suggested the idea that the acid kept it longer, and led to some experiments, which were begun on the 23d; and the results were, that the infusion of the bark alone became fœtid in a few days, the cinchonic odour was lost, and it had no taste of the bark. Others, with addition of vitriolic acid, retained their taste and odour, even for months afterwards. In all of them, however, a fungus or mouldy flake formed on the surface.—Those with the addition of bueyari, ginger, liquorice, together with the acid, remained least changed. That with lemon-peel was more acescent, owing probably to the mucilage of the peel. The liquorice covers the acerbity of the infusion and retards decomposition.

As to the respective values of the two kinds of bark in common use, the pale and yellow,—although the pale is said to

* I neglected to notice, at the time, what kind of bark the sample was from with which I made these trials, as when I commenced it was not with intention of experiment: I can only say, it possessed all the sensible qualities of the very best bark: (probably the pale). It was from Apothecaries' Hall, Glasgow, and was sent out for Plantation Reliance, Essequibo, in 1826 or 1827.

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contain little or no quinine, it appears to be by far the most efficacious medicine, when genuine, or when found to possess the true cinchonic aroma, bitterness, and astringency; for there are very different qualities of bark met with under this name, it being (although referred to *C. lancifolia*) derived indifferently from several other species*. It is this, the pale bark (*Cascarilla fina* of the Spaniards), which is ordered in the Pharmacopœias, and preferred by the *most judicious* British as well as Spanish and American practitioners,—by all, indeed, who form their estimate from extensive experience, unbiassed by the prevailing mania after the *new remedies*, as they are called. By such, however, we find the contrary maintained, both at home and abroad, and that the yellow bark must be best, because it yields the vaunted alkaloid *quinine*, whilst the other affords little or none.

In France, the quinine is cried up as vastly superior to the analogous alkaloid cinchonine,—an opinion which will, of course, be more or less disseminated here. It must be remembered that the *quinine* was a French discovery, and an extraordinary lucrative one; whilst the other nearly identical element (which may be with more propriety termed a *discovery*) was *previously* made by a British physician, as noticed before.

We observe the following notice of cinchonine in a late French work:—"Ses vertus étant inférieurs, diton, de quatre ou cinq fois à celles de la quinine, on ne s'en sert guère en médicine, non plusque des composés dont elle fait la base."—*Pharm. Univ.* 1828, p. 414.

I shall only observe here, that the well known and esta-

* Whoever would acquire the best information respecting the botanical history of this important genus, should consult especially the *Flora Peruv.*, and that valuable and splendid work, Lambert's *Illustrations of the genus Cinchona*. This munificent botanist has described and figured numerous species of this genus. The herbarium, presenting a still greater number of distinct and well-defined species, of which Mr. L. had the good fortune to become the purchaser, together with those derived from various other sources, form altogether a collection which, in the hands of so liberal a possessor and patron of science, may be regarded as a public treasure of great value, as it is freely opened for the inspection of men of science. Dr. A. T. Thomson has also a noble collection of specimens of this genus, which will not a little enhance the value of his instructive lectures on the important science of medical botany, at the New London University.

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blished superiority of the pale bark, which gives no quinine, is a poor presumptive proof in support of such pretensions. Here the quinine has been prescribed in larger doses than cinchonine. Mr. Brande remarks, that cinchonine has been less employed in consequence of the comparative cheapness of quinia, an analogous principle, &c.—See "Man. Ph." p. 62, et seq.

Previous to describing the method I propose, it is proper to observe, in respect to the bark in powder, that its sophistications are so great, that we rarely meet with it genuine in the shops. Besides, all barks and vegetable substances keep best entire, or not powdered; and they can then always be ascertained if genuine or not. Regarding the sophistications of Peruvian bark, Dr. Thomson says—"Practitioners ought never to purchase bark in a state of powder, for in this state it is always found more or less adulterated." Dr. Paris (Pharmacologia) mentions, that "in a late inspection of the shops in London, the censors repeatedly met with powdered cinchona, having a harsh metallic taste. A less injurious, but equally fraudulent mixture, is the powder of bark, which has been employed in making the extract."—*London Dispensatory*, p. 247—as also the dregs, it might be added, after making infusions, decoctions, and tinctures.

Therefore bark should always be ordered out in the gross or natural state, as brought from South America, excepting a small quantity for present use, to be sent by a trusty correspondent.

The experiments of Fabroni, moreover, prove, that protracted pulverization and exposure to the air have a similar effect on bark to that of long boiling; that is, in a certain degree to fix its principles or diminish its solubility, and, consequently, its activity. He obtained 14 per cent. of extract from cinchona simply bruised; whilst from that reduced to very fine powder he obtained but half that proportion, or 7 per cent. of extract!

"This (the powder) is the best form of the remedy. If it excite nausea or vomiting, or operate as a cathartic, or occasion costiveness, these inconveniences may, in some degree, be obviated by combining it with aromatics, opium, or a cathartic, as circumstances direct."

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"Mutis, conceiving that fermentation is the best method for extracting the active part of cinchona, had proposed to make a beer of it, by fermenting 1 part of the bark in powder, with 8 parts of honey or sugar, and 80 or 100 of water. And Alibert, having persuaded a brewer to make some beer with cinchona, administered it to convalescents weakened by protracted intermittents, with the best effects."—*London Disp.*, p. 247. To the utility and efficacy of this method I can bear testimony.

Here I may observe, that certain eminent writers on Pharmacy have asserted, that the Angostura bark (*Galipea officin.*) does not cure intermittents. See "Edin. Disp.," "Brande's Manual of Pharmacy," &c.—Possibly, in the usual mode of employment, this may be correct; but, from adequate experience, I can assert, that a preparation analogous to that mentioned below, as employed at Angostura, is the most successful remedy both for remittents and intermittents—and those, too, of the most malignant and dangerous kinds, in which the cinchona, alone, is well known to be worse than useless. In such cases, therefore, the former is much to be preferred.

During three years that I sojourned in the Orinoko, I had frequent opportunities of witnessing the superior efficacy of this preparation, not only for convalescents, but in the most obstinate bilious and malignant intermittent and continued fevers which raged in that country in 1816 and 1817. The cinchona, being extremely scarce, we had recourse to the native bark, just alluded to, Kina de Caroni, so called, in the military hospital, of which I had charge, having, in all, nearly 200 patients, besides great numbers in the town of Angostura.

The Cervesa de Kina (bark beer) was prepared by the natives in large jars, placed in the sun, with sugar or molasses, a piece of bread, and yeast at times, to hasten the fermentation. It seemed to me, that the caroni bark thus prepared equalled the cinchona in the cure of common intermittents, and certainly much excelled it in malignant fevers and dropsies, diseases which were surprisingly prevalent and fatal throughout the province in 1817, combining at once the miseries of war, famine, and pestilence. This, perhaps, was also partly owing to the

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depressing passion of fear and perturbed spirits, the place being then besieged by the patriots. The preceding was the method chiefly adopted with both these barks in Spanish Guiana *.

I at first imagined, that fermentation would deteriorate the powers of the bark; but experience soon removed my prejudices. I found withal, that the bark retained its bitterness, its slight astringency, and peculiar odour in this infusion. It would seem that its active constituents, far from being subverted, are very materially increased in power by this method; and there can be little doubt, that its alkaline and febrifuge elements will be evolved and exalted by means of the solvent power of the acid and alcohol which is formed in the process of fermentation. We have reason also to suppose, that the antiseptic powers of the bark are much increased in this preparation; and this, if true, would especially recommend it in foul ulcerations, and in all putrid diseases. It appears to be a very eligible preparation in hospital practice, or where the bark is constantly wanted in considerable quantity; but may be less convenient in individual cases, as it requires some time for the preparation, and may succeed best in some quantity. Of all preparations of the bark, it is certainly the least unpleasant, being, in all respects, a species of ale or beer.

The addition of the bueyari bush rope (*Aristolochia Guianensis*) augments the efficacy of the bark in a very eminent degree. I have given the bark, mixed with the infusion of this bitter rope, with marked advantage; but, more recently, since the idea occurred to me of the peculiar uses of the vitriolic acid as eliciting the febrifuge principle, I have prescribed it in a compound infusion, after the following formula.

Take of good Peruvian bark, well bruised, or recently powdered ℥j.
Bueyari stem, well bruised, ʒij.
Ginger root, powdered.
Orange or lemon peel, each ʒj.
Liquorice root, powdered, ʒ iij.

To these ingredients, add first a small glass of brandy or other proof spirit, with ʒij of dilute sulphuric acid or elixir of

* The same plan was pursued with the sarsaparilla, with which they used to prepare a beverage of extraordinary efficacy in venereal and rheumatic disorders, for they rarely made use of mercury. It was, indeed, the best kind of sarsa, fresh and genuine from the Rio Negro.

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vitriol previously mixed; and stir up the mass. Then add half a pint of boiling water, stir it, and keep the vessel closely covered, near the fire, at a scalding heat for an hour or two, according as the ingredients may be more or less reduced or pulverized; as the coarser they are the more time is required for infusion. A pint of boiling water is now to be added; and the digestion continued for an hour or two more, stirring it occasionally, or shaking, without uncovering it. The infusion may afterwards be strained, well pressed out, and bottled, adding two or three bruised cloves, or a drop or two of the oil. —As the pale bark contains chiefly cinchonine, which is said to be most soluble in the muriatic acid, this, if preferred, might be substituted for the sulphuric.

Two or three ounces of good bark, prepared in this way, will go as far, in the cure of intermittents, as two drachms of quinine; and, as a general tonic and restorative remedy, its power is vastly superior to quinine, and may be considered as inferior to no tonic whatever.

The efficacy of the formula is improved by the addition of 4 or 5 grs. of ipecacuanha along with the other ingredients, the beneficial effects of which, on the digestive functions, are much greater than might be expected from so small an addition. This compound is best calculated, both for arresting the progress of intermittents, and for a general tonic in weakness of stomach and loss of appetite, and it is especially useful in remittents, in typhus, and malignant forms of fever. By strengthening the stomach and bowels, it proves the most effective remedy against the breeding of worms in children, *i. e.* after their expulsion by cowhage, or other proper vermifuge; the dose in such cases being a tablespoonful or two, according to the age of the child.

The reason for employing but a small quantity of water at first, is to avoid overdiluting the acid, by which its energy, as a solvent of the quinine or alkaloid, might be rendered less efficient. It is also of much importance first to moisten the ingredients with a little spirit. It serves as a medium to produce a far more perfect combination of the active elements of the compound with a watery menstruum, than could possibly be effected without it.

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This method, it is presumed, may be found very useful in similar pharmaceutic operations, as combining to a considerable extent, and at a trifling expense, the advantage of both the spirituous and aqueous menstrua. Since the idea occurred to me, I have tried it on various other substances. It is necessary to witness the effect, in order to believe how much this small quantity of alcohol, *previously added*, will forward the evolution of the more active principles of vegetables. By this method of preparation we are sure of retaining all the active elements of the bark in solution.

The infusion may be taken in the dose of a wine-glass full once in two, three, four, six, to twenty-four hours, according to the intention or urgency of the case in hand *. When

* I may here notice a fact which has often struck me forcibly, viz. that the vegetable infusions in general, whether febrifuges or general tonics, cathartics, alteratives, &c., are found to be sensibly more efficacious when taken *warm*: there is one direct proof I can adduce of this position in the well known action of infusion of chamomile.

This we find prescribed in a cold infusion as a stomachic. (Brande's "Manual of Pharmacy.") Its emetic property, taken in a tepid state, is well known. I have been told, that the country-people, in certain parts of England, as about Wootton, in Oxfordshire especially, frequently use a strong infusion of chamomile as a cathartic, taking it at bed-time, as hot as it can well be swallowed, and that, in this way, it does not produce emeses, but, as we should naturally infer, acts presently as a potent sudorific; and the following morning as a gentle but effectual purgative. This is a very common resort, as I am assured by a young man acquainted with that part of the country, Mr. E. Charles Prince; and it is there considered as one of the best of remedies in indigestions, colic pains, and obstructions of the bowels, especially when arising from cold, which we know to be the more common exciting cause of those disorders.

To this I may add, that the caroni bark (*Galipea off.*) is employed in a fresh state by the natives on the place of its habitat, precisely in the same manner, and with the same effects, as just noticed of the chamomile; and analogous to this,—a cup of strong coffee, taken hot on an empty stomach, will often produce a similar effect, and that very speedily and pleasantly, '*cito, tuto, et jucunde*'; in respect to time, indeed, presenting a singular anomaly with *the tardy* operation of the chamomile.

This, perhaps, is explainable, with respect to vegetables in general, mainly upon the well-known fact, that hot water will take up and hold more of their active principles in solution, than it can do when cold; yet, this is probably not the only reason,—it is partly owing, no doubt, to their more perfectly assimilating and entering the circulation, in this more perfect state of solution; at all events it is certain, that the temperature, in which we exhibit the vegetable infusion, appears to be a matter of more importance than is generally imagined.

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the infusion is strained, (as the virtues of the ingredients are not yet exhausted,) another pint of boiling water may be added and digested upon the dregs. This second infusion may be taken with more freedom.

We thus have a preparation to be depended on, as containing all the active properties of the bark, even its aromatic parts, in a form almost as pleasant, and as easily swallowed as a glass of beer. That the preparation here offered must contain all the energetic properties of the bark, is fully evident, if the sulphuric acid does really dissolve and develop from the bark an alkaline base, or febrifuge element, as cinchonine and quinine; and of this no doubt can be entertained, as otherwise all that has been said of such elements must be nought but sheer pretence and quackery *.

* This paper is composed from notes, written at various times, but chiefly in the year 1827. Since which, I have employed the compound infusion of cinchona, in numerous cases, with the most decided success, in my own family and that of others, both in Demerara and since our residence in England; and having tried it under many variations of the formula, I give the decided preference to the foregoing prescription.

The infusion of the bark, with a small portion of acid and alcohol, prepared in this way, is found to possess all the flavour and odour of the bark in powder, and I doubt not it contains its entire medicinal properties; and by an operation sufficiently simple,—free from the absurd processes so justly deprecated by Dr. Paris, in the preceding quotations; and not only are the entire virtues of the bark thus evolved, but they are much enhanced when the additional articles are employed, as the bueyari or quassia, ginger and orange peel, as well as the acid. If desired to diminish the bulk, and render the compound more portable, when required for carriage especially, it is only requisite to increase the quantity of spirit, and diminish that of the water—in short, to employ proof spirit or a weaker menstruum, at the option of the practitioner.

It is to be considered, that spirituous menstrua will take up a larger portion of the active elements than an aqueous one can do. Upon the foregoing principles, therefore, we shall be enabled to form a very concentrated tincture of this sort, to the exclusion of some inert vegetable mucilage, which a more watery menstruum must hold in solution. If proof spirit be employed, the menstruum need not exceed the proportion of ½ the quantity of the *infusion* above stated, using all the same ingredients and their quantities as above. It may be digested with a little heat in a bottle lightly corked.

A very small portion of acid is required to neutralize the alkaline principles of the bark. See "Edinburgh Medical and Surgical Journal," vol. xviii., p. 152, and table 155, where it is stated that cinchona combines with sulphuric acid, in the proportion of 100 parts to about 13 of acid, and kina with 11 parts of acid; on an average of the two, about 100 of the base, or alkali, to 12 of acid. It

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In this acidulated infusion, we obtain all the elements of the bark, excepting the woody fibre, and perhaps a small portion of resin; whereas, in the process for preparing quinine, the tannin of the bark, which gives it a light styptic or astringent taste, goes past with the liquor or decoction, together with much of the native proper bitter of the bark, as found by experiment, the gummy extractive, resinoid colouring matter, and kinate of lime, while the essential oil, in which the aromatic principle resides, is necessarily dissipated in the

It has been found that the bark, exhausted of its soluble parts in the preparations of extracts, decoctions, &c., still yields nearly an equal portion of the alkali, or quinine. (See "Dr. Johnson's Med. Chir. Journal" for October, 1825.)

Dr. Paris, vol. ii. p. 249, has cited the "Edin. Med. and Surg. Journal," (*vide* next page.) and in some measure seems to have given into the error there noticed. "With respect to their physiological action, it may be stated, that they (the alkaloids) would appear to concentrate in themselves the characteristic properties of the vegetables to which they belong; and yet, although their effects are much greater than those of the undecomposed vegetables, the same quantity of alkali is not so powerful in its pure form, as in its natural state of combination. Thus 1 gr. of morphia produces no more effect than 2 grs. of Turkey opium, which does not contain more than a sixteenth part of the alkali. To explain this loss of efficacy which usually attends our attempts at concentration, the reader is referred to the observations which have been already offered upon this subject in the first volume of this work, page 283."

If, then, 1 gr. of morphia be only equal to 2 of crude opium, and if 16 grs. must be expended to procure the grain of morphia! the concentration certainly does not appear so very striking, nor the effects so much greater than in the undecomposed vegetable. It appears, then, that a sort of *Hibernian* gain alone, is the result of the tedious process for procuring morphia!

A case is related, in a recent number of the "Bull. Univ." of a person having swallowed 24 grs. of acetate of morphia and recovered, although no medical aid was afforded for ten hours, and it was probably superfluous, as its sedative or narcotic effects must then have been *spontaneously* subsiding. This shews how far the vaunted concentration and pretended power is to be depended on: had it been of *crude opium* the case would, doubtless, have ended differently. Such facts deserve to be duly reflected on by the more cool, judicious and philosophic chemist, amidst the popular mania after morphia, quinine, &c.

The discovery of new vegetable elements, now forms an epocha, equal to the recent tide of discovery of new metals and minerals. A most admirable phraseology has been adopted by certain sagacious discoverers. —"Huttenschmid y a decouvert deux substances nouvelles, in Cortex Geoffroyæ probablement de nature alcaline, a aux quelles il a donné les noms de *Jamaicine* et de *Surinamine.—Pharm. Univ.* vol. i. p. 606.—All the colonies, islands, &c. will thus furnish a most elegant and appropriate nomenclature for the new principles to be discovered!

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boiling. How, then, could it ever have been imagined for a moment, that *a single insulated principle* could possess the exalted virtues arising from a combination of the *various active elements* of Peruvian bark* ? . . . . . .

The results derived from the use of this preparation, in intermittents, in many cases of general debility, &c. give me reason to consider it as the most effectual and certain preparation that can be devised, either as a febrifuge, or general tonic, and I may say corrector and alterative in loss of appetite, weakened state of the stomach and digestive organs, and their concomitant evils, as nervous weakness, spasms, gout, dropsies, ulcerations, scrofula, tubercular consumption, leucophlegmasia, &c. in such cases, it forms a most valuable remedy, taken alternately with the muriated tincture of iron. In those chronic maladies, it would often be of advantage to increase the quantity of the acid.

From the trials I have more lately made with it, I am fully convinced, that this preparation is a far more effectual febrifuge than the same quantity of the best bark taken in substance or powder. It always sits easy on the stomach, and is never

* Such reflections could not have been adverted to by the learned editor of the "Edin. Med. and Surg. Journal," when he expressed himself at a loss to account for the inferiority of these alkalies to the entire or undecomposed vegetables. "In employing the alkalies instead of the vegetables, not only is the expense augmented by the process for their preparation, but they actually do not possess nearly the same degree of energy as they do in their natural state of combination; that is, if an ounce of any vegetable matter contains 30 grs. of an alkali, those 30 grs. are not so powerful in their pure state as when they are united with other principles, to form an ounce of the undecomposed vegetable." To these judicious remarks, he strangely adds, "We have no means of conjecturing whence this proceeds." Vol. xviii. p. 162; and at page 155, it is remarked, that "These vegetable alkalies are endowed with a powerful influence upon the animal economy, and it is a law, hitherto universal, that they concentrate in themselves all the physiological properties of the vegetables, to which they belong! It is worthy of remark, however, that although their effect is much greater than that of the undecomposed vegetables, the same quantity of alkali is not so powerful in its pure form as in its natural state of combination." Nothing could be more just than the last sentence, whilst it is a direct contradiction to the preceding one. It is curious to observe the conflicting opinions which arise from false premises; or does this originate in an undue deference to the *Magni Nominis Umbra*—an overweening confidence in annunciations emanating from men of high celebrity?

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disgusting, especially if at the end of the operation, you agitate with the infusion three or four drops of the essential oil of cinnamon, or of cassia, dissolved in a little brandy.

One ounce of good bark, prepared in the foregoing manner, will generally be found sufficient to remove an intermittent, especially if taken warm, and freely, just before the expected return of the paroxysm. The cost of the articles will be a mere trifle. In want of the bitter rope, the lignum quassiæ will be found a good substitute, and may be used in the same proportion *.

In conclusion, I must apologize for the length and imperfections of this paper; I can only hope it will serve as an inducement to abler minds, to investigate the important subject of which it treats.

J. H.

* Huxham's celebrated prescription, which still holds its credit in medical practice, was a vinous tincture of the bark with orange-peel, snake-root, saffron, and cochineal, which he directed to be given in wine and water with addition of elixir of vitriol, and which, he observed, he had long used with success in intermittent and slow nervous fevers, as well as in the putrid, pestilential, and petechial. He says, that "although very profuse sweats may be hurtful in those and other fevers, yet gentle and moderate sweats are ever to be encouraged by diluents. And, as these fevers are frequently long protracted, and thus exhaust the patient, it is requisite to support the strength with liquid nourishment and generous old wine, acidulated with lemon juice, and impregnated with aromatics, as cinnamon, orange-rind, red roses, and a few drops of elixir of vitriol, &c." He adds, that "the Asiatics and other nations, among whom pestilential disorders are much more rife than with us, lay more stress on the juice of lemons than on the most celebrated alexipharmic." (See "Huxham on Fevers," p. 122.)—This remark is verified with respect to the popular treatment of these disorders amongst the French and Spaniards in the tropical parts of America; and this practice, together with the use of baths and frictions, is attended with much greater success, than that which results from the common European practice with calomel, antimonials, strong purgatives, &c.

The compound tincture, with elixir of vitriol, Huxham called an excellent *antiseptic alexipharmic*, which it really is; but there can be no doubt its efficacy would be much augmented by *previously* adding the acid.

Though the foregoing remarks may be thought not entirely relevant here, yet it is hoped that the experience of Dr. Huxham will be a sufficient apology for the brief mention of his opinions.

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Some Notices of the great Storm and Flood which occurred in the Counties of Aberdeen, Banff, Elgin, and Nairn, and parts of Mearns, Angus, Perth, and Inverness, on the 3d and 4th August, 1829.

[In a Letter from the Rev. James Farquharson, of Alford, Abeerdeenshire, communicated by Captain Sabine.]

At the period of this storm I was residing temporarily at the village of Ballatu, situated on the river Dee, about forty miles westward from Aberdeen; and the house in which I lodged is the nearest to the river and bridge which was swept away, having from the windows a full view of both within forty yards.

On the day preceding the storm, the weather was mild and warm, and the sky, shrouded by dense strata of clouds moving slowly eastward, giving indications rather of the approach of a westerly gale than of the terrible catastrophe which followed.

The morning of the 3d August was ushered in with a drizzling rain, with light wind from a point east of north. About 9 A. M. the river began to be slightly discoloured with flood-water, indicating more rain in the mountains than we yet had in the valley. About mid-day the rain intermitted for two hours, but came on again before 2 P. M., and the wind shifted to a point west of north, the river slowly and uniformly rising all the time; but up to 5 P. M. there was nothing unusual either in the force of the wind or quantity of the rain.

From about that hour the elements suddenly assumed all the characters marked in the common descriptions which we have received of the tropical hurricanes. The wind blew with extreme violence in sudden gusts, and whirlwinds, whose centres were well defined to the eye, as they crossed and vehemently agitated the surface of the river. It is, probably, similar whirlwinds which are described to us, when in the accounts of the tropical hurricanes we are informed, "that the wind blows from all points of the compass at once." The centres of those in question all moved forward in parallel paths from a point west of north; and so great was their force, that having occasion to give orders about my horse at a place about 150 yards from my

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lodgings, I had several narrow escapes from being violently thrown down, or dashed against the neighbouring houses.

A very heavy rain now fell without intermission; and at half-past five, lightning and thunder began, and continued, with short intervals between the flashes, till 8 P. M., when it ceased. The lightning, with the exception of the two or three first flashes, which were similar to the ordinary forked lightning of this climate, followed by acute reports, was of a character which I have seldom witnessed. It presented the appearance of extensive blazes or sheets of flame dispersed over a large space of the heavens; and, although it was often very near, was followed by no acute report, but by a grave rumbling noise like the rolling of distant drums, passing often into a hissing sound like that of sky-rockets.

There was no sensible intermission of the rain till about 9 P. M.; after which it consisted of a succession of showers, very gradually diminishing in heaviness, and coming on after gradually increasing intervals of time, till 6 A. M. of the 4th, when it ceased entirely. The wind abated with the rain.

The river rose gradually but steadily, after 6 P. M. of the 3d, till 11 P. M., when it was about 8 feet above its usual level. From that hour, till 12 P. M., it rather fell; but towards 1 A. M. of the 4th, the flood-water of the head river in Braemar, which drains an extensive valley among the highest Alps of Scotland, by a number of branches uniting like the radii of a circle, and passes through a narrow defile at Invercauld, about 20 miles higher up, having at length reached Ballatu, the river again rose with great rapidity. For a short time between 1 and 2 A. M. the rise was at the rate of one foot in ten minutes, as I had an opportunity of ascertaining in the lower story of the house where I lodged, which was by that time inundated; and the ultimate rise of the river, which occurred about 5 A. M. was at this place about 13 feet above its ordinary level. The greater part of the inhabitants of the village, which is about 11 feet above the river, were driven out of their houses, which are mostly of only one story, at 2 A. M.

The bridge at this place, built of granite about twenty years ago, consisted of five arches, having a water-way, in whole, of 260 feet. As the bed of the river consists of rolled pieces of

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granite, and gneiss of unknown depth, the piers of the bridge had been laid on platforms of wood, let down about 5 feet below the bed; and this expedient had been sufficient for bearing the vertical pressure, and up to the time of this flood there was no settling or flaw in the masonry. It became apparent, however, before dark on the 3d, that there was not sufficient water-way for this unusual flood; and a damming back of the water took place at the bridge, which caused a sudden fall of about 4 feet perpendicular into the openings of the arches. The height of this fall had, no doubt, increased with the rise of the river through the night, but could not be seen for the darkness. The effect was, that the rolled materials of the bottom were swept away, and the piers were at last undermined. The upper half of the north pier fell up the river about 3 A. M. of the 4th, letting down parts of the contiguous arches. The bridge remained in this state about an hour and a half, when the south pier fell wholly up the river, letting down the two south arches; and about ten minutes afterwards, the centre arch and remaining portion of the two north arches fell, apparently, from the spreading of the piers by the lateral pressure of the arches. The materials of this great mass of masonry have, in a great measure, disappeared in the bed of the river, being, no doubt, buried in the deep pit, scooped out under it before it fell.

The ordinary depth of the river at the shallows near the bridge, is from 2 to 3 feet, and the fall, in its course, about 20 feet in a mile.

This great storm and flood extended, it seems, nearly simultaneously, and in equal violence, over a space of about 5000 square miles;—being that part of Scotland cut off to the north-eastward, by two lines, from the head of Lochrannoch, one towards Inverness, and another towards Stonehaven*. All the rivers within that space were flooded, proportionably, with the Dee. The damage suffered, in consequence, in the destruction of bridges and roads, and lands, buildings, and crops, along the courses of the streams, has been very great, and has been estimated at half a million sterling. Happily few lives were lost.

* The storm extended much farther, but without the great rain.

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The effects of the storm on the grain crops, excepting where they were overflowed by the streams, were less injurious than its severity might have led one to anticipate. They had not yet passed the full vigour of vegetation, the ears being, in few cases, fully shot out; and a remarkably fine day with much sun, on the 6th of August, enabled the plants to rise up again; and, ultimately, a harvest, not very deficient, was reaped in all the more productive districts.

Being from home, I had no opportunity of observing, myself, the state of the barometer or thermometer at the period of this flood; but have learnt since, from unquestionable authority, that nothing remarkable was indicated by either. At the commencement of the storm, the barometer was a little above its mean height; and its fall, during the progress of the storm, was trifling. The thermometer was nearly at temperate all the time. I have learnt from Mr. Murdock, at Huntly-Lodge, who registers a rain guage, that the fall between 5 A. M. on the 3d, and 5 A. M. on the 4th, was, at that place, $3\frac{3}{4}$ inches. The larger proportion of this fell between 5 and 9 P. M. on the 3d.

The effect of running water, in moving stones and heavy materials, has been, in many instances, curiously illustrated by this flood. The River Don has, upon my own premises, forced a mass of 400 or 500 tons of stones, many of them 200 or 300 pounds weight, up an inclined plane, rising 6 feet in 8 or 10 yards; and left them in a rectangular heap, about 3 feet deep on a flat ground; and, singularly enough, the heap ends abruptly at its lower extremity. A large stone, of 3 or 4 tons, which I have known for many years in a deep pool of the river, has been moved about 100 yards from its place; and an immense collection of small stones, of many hundred tons, gathered from the arable land, and laid down as an embankment at a turn of the small river Leochell, has been carried down, and left on the field below, not promiscuously dispersed, but as if artificially collected into large and deep heaps.

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Observations on the Teeth of the Erinaceus Europæus, Urchin, or common Hedgehog.

To the Editor of the Quarterly Journal of Science.

SIR,

ALREADY, more than once, I have had occasion to notice the uncertainty, as primary distinctions, of those popularly favourite zoographical characters, the teeth, which are found, not only to associate animals essentially dissimilar, and to separate others as intimately allied, but also to be so ambiguous in themselves, that the same individual has been placed in different groups, not only by different persons, but even by the same zoologist at different times: the Manupeda and Alipeda have afforded us examples, and the other types will give their testimony also. But, perhaps, as a solitary proof of this perplexing ambiguity, there is not a more striking or notorious instance to be met with, than in the very discordant accounts which several most distinguished naturalists have given of the dental system of the Erinaceus Europæus, or common hedgehog; and which, as it is included in the following outline of the arrangement of the Quadrupeda, demands, in the first place, our attentive consideration.

Linnæus, Brisson, Pennant, Shaw, Blumenbach, George Cuvier, Illiger, Desmarest, and most zoologists, describe the hedgehog as possessing, and, indeed, characterized by the possession of canine or laniar teeth, though they vary in their statements as to the especial number: while the same Pennant, Frederick Cuvier, and Latreille deny the existence of any canines at all. On this subject, Pennant writes, "it is impossible to allow the harmless hedgehog to be the companion of lions, wolves, and bears;" yet such is the common arrangement, and such the arrangement that he afterwards adopted. G. Cuvier says, "les Hérissons, &c., *ont le canines plus courtes* que les antres dents;" and Illiger describes them as ambiguous; instead of being, as Shaw declares, "*perfectly* those of the order Feræ." The following tabular conspectus will shew the difficulty of discrimination from the extraordinary discrepancies which exist among the most astute zoologists: it will also prove that the same teeth have been, by different

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authors, called, at one time, incisors, and at another canines; that the canines, in like manner, have been occasionally denominated molars; and again, that even to the same teeth all the three several epithets have been indifferently ascribed.

	incisors	canines	recumbent.
Linnæus	$\frac{2}{2}$	$\frac{2-2}{2-2}$	
Pennant	$\frac{2}{2}$	$\frac{0-0}{0-0}$ or $\frac{4-4}{3-3}$	molars $\frac{6-6}{4-4}$
Shaw	$\frac{2}{2}$	$\frac{5-5}{3-3}$	$\frac{4-4}{4-4}$
Blumenbach	$\frac{6}{6}$	$\frac{3-3}{1-1}$	$\frac{4-4}{4-4}$
Illiger	$\frac{6}{6}$	$\frac{2-2}{1-1}$	$\frac{5-5}{4-4}$
George Cuvier	$\frac{6}{6}$	$\frac{2-2}{1-1}$	$\frac{4-4}{4-4}$
Frederick Cuvier	$\frac{6}{2}$	$\frac{0-0}{0-0}$	$\frac{7-7}{7-7}$
Griffiths' Cuvier and Desmarest	$\frac{6}{6}$	$\frac{1-1}{1-1}$	$\frac{5-5}{4-4}$
Specimen	$\frac{6}{6}$ or $\frac{6}{8}$	$\frac{3-3}{1-1}$ or $\frac{2\,.\,2}{0\,.\,0}$	$\frac{4-4}{4-4}$ or $\frac{5-5}{4-4}$

Had these contradictory accounts been published of some rare animal, the paucity of examples submitted to each inquirer, and the casualties to which all organized beings are liable, might have explained the variations, or, at least, have proffered an excuse for the inconstancy of the accounts; which now must be confessed to be owing to the questionable characters of the teeth themselves. Still, as the doubt has directed the attention of naturalists especially to this point, it is curious that the same objects should have been so variously described, that the same premises should have led to such different conclusions.

The dental developement of the hedgehog, when critically and scrupulously examined, is perceived to adumbrate with one system of teeth, the type to be perfected in another; and this so closely, that perhaps it might, by the captious, still be questioned to which group we should refer them. Superficial observation might be led by the spinous integument to connect them with certain rats, or rather with the porcupine, to which

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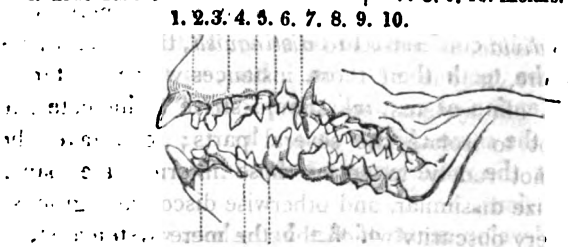
they are undoubtedly, in some respects, allied; but their habits of life and food, certainly, associate them much more closely with the subterranean insectivorous Feræ; and they are, like them, beasts of prey, as far as their strength and circumstances permit. In tracing the dental degradation alluded to, it must be borne in mind, that the front teeth, which, in the Feræ, and many other beasts, from their conical shape, little fitting them for cutting, have been called dentes primores, instead of dentes incisores, and which are usually shorter than the canines or laniar teeth; in the majority of cases are set side by side, forming a line in front of the mouth; in the hedgehog have their position reversed: two in each jaw being advanced in front, and greatly elongated, while four others follow; two, at least, on each side the mouth in either jaw, and almost recumbent. The canines are certainly ambiguous both in shape and number, being shorter than the long incisors, and having, in the under jaw, somewhat the form of bicuspidor false molar teeth; and, by Frederick Cuvier, they have so been named. From their position, however, and their single fangs, together with the predaceous habits of the animal, they are more properly considered as transitional canines; indeed, two in the upper jaw decidedly retain the laniar form, while, in the lower jaw, there are, as certainly, no true canines at all.

1. The Rodent Incisor of Upper Jaw.
2. 3. Degenerate Incisors.
4. True Canine.
5. Transitional, or
6. Degenerate ditto.
7. 8. 9. 10. Molars.



1. 2. 3. 4. 5. 6. 7. 8.

1. Rodent Incisor of Lower Jaw.
2. 3. Transitional Recumbent do.
4. Ambiguous Tooth in the place of the Canine, accompanying the incisors, and having a Molar form.
5. 6. 7. 8. True Molars.

Thus it becomes manifest, that the teeth described by Linnæus as canines, two on each side recumbent, are, in truth,

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degenerate incisors; and the anterior of his molar teeth are the real transitional canines. Shaw's canines included the recumbent incisors, together with the canines and anterior molars of Pennant and Linnæus. Blumenbach distinguishes the incisors correctly, but his canines include the anterior molars of Illiger and the elder Cuvier; while Frederick Cuvier includes among his molars, not only the canines of the other writers, but even the two hindmost incisors on either side of the lower jaw.

Hence the hedgehog forms an admirable illustration of the transitional structure, which connects two tribes of quadrupeds, whose habits and dispositions are essentially distinct, the Rodentia and the Feræ; the one a race of harmless, almost defenceless, herbivorous beasts; the other a series in which power and ferocity are developed to their full extent: and yet so gradual is the change from these to those, from the lion, the tiger, and the sanguinary wolf, to the mouse, the squirrel, and the timid hare,—from those which have the tearing teeth large, strong, and prominent, to these in which they are obsolete, obscure, or absent, and even to such as have no teeth at all; that in the osculant genera, as the shrew, the hedgehog, and the mole, their presence is alternately admitted and denied; at one time associating them with the gnawing beasts, at another with the beasts of prey.

This circumstance, which hath, as might have been supposed, caused much perplexity to such zoographers who would seek to *divide*, as well as to *distinguish*, the several provinces of nature, is one of those instances which go far to prove that, in our systems, we should strive to indicate the connexions, not to isolate the several parts; to unravel the associations, not rudely to sever those intermediate bands which harmonize dissimilar, and otherwise discordant groups; for then the very obscurity which, by the mere systematist, is esteemed a stumbling-block, to the physiologist becomes a pharos, and is regarded not only as an interesting, but an important and valuable guide.

I have the honour to remain,

Yours obediently,

GILBERT THOMAS BURNETT.

Theatre of Anatomy, Great Windmill Street.
November 30, 1829.

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Illustrations of the Quadrupeda, or Quadrupeds, being the arrangement of the true four-footed Beasts indicated in outline.

Tetrapoda, Quadrupeda, the originals of our naturalized Tetrapods and Quadrupeds, words formerly synonymous, and equally translatable four-footed, have, however, in the progress of science, become appropriated to different meanings; and this is one of the advantages which our language gains by engrafting into it classic terms; for shades of difference are thus succinctly and elegantly expressed without a periphrase; an important power, of which dialects, less mixed, are utterly unconscious and devoid.

The earlier naturalists, to whom little more than the external structure of animals was known, and by whom dissections were but seldom practised, consequently selected for their zoographic signs the most obvious features of exterior form, such as the wings, the feet, the claws, the teeth, &c., aided, when observation failed them not, by the habits of the several grades. Hence the Tetrapoda of the Greeks, and the Quadrupeda of the Latins, were terms literally and indiscriminately applied, and included the four-footed reptiles, along with the four-footed beasts. The frog, the toad, the tortoise and the crocodile were associated, in one compartment, with the lion, the deer, the elephant and the mouse; the two series being inter-distinguished as the oviparous and the viviparous sections. Yet, as further investigations shewed, not only that these were more or less clothed with hair, while those were invested with scales, or naked; but also that the one had warm blood and a double heart, the others a single heart, and varied in their temperature with the medium around them; that the former were more nearly allied to serpents, and the latter to whales, than either to each other; the number and developement of the feet proving fallacious, as a primary distinction, was very properly eschewed; and other less defective means proposed and followed in their stead.

The discovery of a proper circulation, which threw so much light on the animal economy, and solved so many problems in physiology, had its influence on physiography likewise; and

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the Linnæan method, formed chiefly on this principle, in great measure superseded the respiratory classification, recommended by Aristotle, and adopted by Ray and others. But the nervous system is the idol of the present day, and to this the circulating seems doomed to yield as the respiratory succumbed to it; for, according to the developement and concentration of the brain and nerves, a primary distribution has been effected by the celebrated Cuvier, who divides the whole animal reign into the vertebrate and invertebrate series; the latter of which is again subdivided, before the distribution of either into classes. Of the successive prevalence of these several schemes, founded on the external or internal structure, on the respiratory, the circulating and the sensorial systems, more shall be said in a future essay; the mere recollection of their epochal predominance is all that will now be useful: each plan has advantages and disadvantages peculiar to itself; all assist in part to explicate the oracles of nature; and it is only when either becomes advanced unduly, that those sections are rendered needlessly obscure, that its compeers are probably fitted to elucidate in turn. Therefore it is an object worthy the attempt, so to reconcile the ancient emphatic terms deduced from external and obvious characters, with the internal and essential structure developed in modern times, that while we retain, as far as possible, the familiar and expressive nomenclature established by the one, the definitions may be corrected to include the important anatomical discoveries of the other. Thus, for example, while all the warm-blooded, hairy, unfeathered brutes are known as beasts, and the cold-blooded, lung-breathing animals are classed as reptiles, the number and form of their limbs, and the popular epithets derived therefrom, although arbitrary and erring as primary, are most convenient and just as subordinate, distinctions; and the essential peculiarities of the intimate structure and constitution of the separate grades is well predicated by calling the four-footed beasts Quadrupeds, the four-footed reptiles Tetrapods; thus making a single expressive word include, not only the obvious external form, but also indicate the physiological peculiarities of their internal structure.

Great as are the varieties of external form, and numerous as are the modifications of internal structure, still there is a rela-

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tive developement of each; and the outward manifestation of the one is, in general, a sign of the inward developement of the other. The resources of nature are, it is true, so mighty, and our knowledge of her scheme, at best, so limited, that this index is not unfrequently mistaken, and on some occasions has been imprudently propounded as *a necessary condition of existence.* Even the illustrious Cuvier would seem to have been too much dazzled with the brilliant theory of this mutual interdependence of the various internal and external organs; not sufficiently calculating our ignorance of the compensating means by which, as Flemming observes, a state of mutual dependence and reciprocity is maintained, in the midst of a diversity of combinations which display, in the most astonishing manner, the endless resources of the wisdom and power of the Great Creator, "whose ways are not as our ways, neither are our thoughts his thoughts." This frequent harmony between the various organs that act upon each other, and this reciprocity which we can often trace between the inner structure and the outer form, has been admirably illustrated by the first-named writer; who observes, "an animal which can only digest flesh, must, to preserve its species, have the power of discovering its prey, of pursuing, of seizing, of overcoming and of tearing it to pieces. It is necessary, then, that this animal should have a penetrating eye, a quick smell, a swift motion, address and strength in the talons and the jaws. Agreeably to this necessity, a sharp tooth fitted for cutting, is never coexistent in the same species with a foot covered with horn, which can only support the animal, but with which it cannot grasp anything; hence the law by which all hoofed animals are herbivorous; and also those still more detailed laws, which are but corollaries of the first, viz. that hoofs indicate dentes molares with flat crowns, a very long alimentary canal, a capacious or multiplied stomach, and several other relations of the same kind." Not to dilate on the many, perhaps only apparent exceptions to this law, which, in the present imperfect state of our knowledge, certainly do exist, enough has been already done to shew that these reciprocal indications abound; and as far as may be, they should always be sought after: still they are not always to be traced, and when they cannot, although an important point of internal

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structure must never be neglected, more especially in physiological discussions, still an obvious external character will be commonly the most useful sign; not only in a popular, but also in every system of zoographical arrangement.

This reciprocal adaptation of external characters to indicate internal forms, and this reconcilement of ancient, established and familiar terms, with modern discoveries and attainments, are objects too little and too seldom regarded; and names both perspicuous and expressive, are too often allowed to become obsolete, or stigmatised as vulgar and incorrect, while others, less energetic and equally objectionable, are as often introduced. Of this, the words *Brutes, Beasts, Quadrupeds, Cattle*, &c. &c. now attempted to be re-established, may serve as apposite examples. Thus the word Quadruped, as above defined, when limited to the truly four-footed individuals of the one class, while Tetrapod is restrained to those of the other, may again become a useful philosophic term, after being all but obsolete in science; though in popular language, from its obvious signification, it has always maintained its ground.

Having, on principles similar to these, already applied the word Brutes, to denote mere animals, thus excluding the mental or human grade, and distinguishing rational and immortal man from the mere sensual brutes that perish, (*vide* "Journal of Science," No. vi.) and having defined the first class of brutes to be those vertebrated, lung-breathing, warm-blooded animals, whose bodies are invested with hair, or naked; at least not covered by feathers; which brute animals we termed Beasts, to distinguish them from Birds and Reptiles, and having (in "Journal of Science," Nos. viii. and x.) followed out the distribution of the two first types of this class, which have respectively feet in the form of hands, and feet in the form of wings, the next series will include all the true four-footed mammalious beasts, which will be treated of under the name of Quadrupeds, thus completing, with the Alipeds and Manupeds, the wing-footed and the hand-footed types, the first order of this first class, all of which are characterised by having four well-developed limbs, either in the form of hands, or wings, or feet, possessing breasts and being viviparous, in these points contrasting, as will be more particularly hereafter shewn, with the two other orders of this class; the

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one of which, as the Ornithorhynchus, although four-footed, is not mammiferous; and the other, as the whales, although mammalious, are not four-footed. The subordinate distribution of the Quadrupeds, or truly four-footed beasts, will depend upon the respective developement of the toes and their relative armature by claws or hoofs; the presence of Marsupia, or the conditions thereof; and subsequently on the characters afforded by the teeth, which, although producing some anomalous associations, when resorted to as a first diagnosis, become most valuable distinctions when used to indicate especial groups.

The quadrupeds, which, as a type, are both mammalious and four-footed, become naturally interdistinguished among themselves by one district, being digitate and having claws: another shewing the toes far less developed, the digits being confined and encased in hoofs: and connecting these two extremes, the hoofed and the claw-footed beasts, there is found an intermediate district, comprising a most curious race of animals, which are naturally, as it were, parturient by abortion, and for the most part furnished with abdominal teat-pouches, resembling, in some respects, an external matrix or living nest; and which præcocinate animals approach, by some of their species, the clawed and digitated, by others, the hoofed extremes of their common type. These three districts are by name the Unguipedatæ (Unguipedates), or claw-footed beasts; the Cornipedatæ (Cornipedates), hoofed or hoof-footed beasts; and the Præcocinatæ (Præcocinates), Marsupiatæ (Marsupiates), pouched beasts, or Mastothecatæ (Mastothecates), teat-pouched quadrupeds; the abdominal sac, by which they are characterized, having been named Mastotheca, or Marsupium. Other names, or modifications of the foregoing, may, and, indeed, have been given to these several districts, still in general they are little more than trifling variations, and scarcely require enumeration even as synonymes. The Unguipedatæ might be called Unguiculatæ, Ungulatæ, or Digitipedæ; the Cornipedatæ, Ungulosæ, or Ungulogradæ; the Præcocinatæ, Marsupiatæ, Eplacentaria, Marsupialia, &c. &c.; but the former terms and terminations are, perhaps, the best; especially as regards the last-named animals, which are all præcocinate, although they are not all possessed of a Marsupium.

The quadrupeds being the largest type of this first order of

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the class of beasts, admit of more numerous subdivisions than either the hand, or the wing-footed *types*; in each of which but one *district* was included, comprising the *races* and the *kinds*; in this, however, three *districts* are found, each containing three several *races*, and each race its appropriate kinds. These districts have been already named the Cornipedates, the Præcocinates or Marsupiates, and the Unguipedates, and of each of them in turn. The indications, however, shall be strictly restrained to outline. (*Vide* Table, p. 348.)

Among the digitated quadrupeds, associated either by having distinctly developed toes, or their digits, if less exserted, being armed with nails or claws, never having hoofs, or Mastotheca, considerable differences in habit and in other natural characters prevail. One extensive race includes the more or less savage beasts, such as are usually referred to as beasts of prey; and the pugnacious and sarcophagal propensities pervade the entire race, though subject to many modifications, according to the corporeal strength of the several kinds, or the circumstances under which certain individuals may be placed. They are strongest in the lion, the tiger, the wolf, the fox, the weazel, and the ferret: weaker in the bear, the badger, and the racoon; and evanishing in the mole, the shrew, and the hedgehog; by which last the Feræ are connected with another type. In the true Feræ and their allies three sorts of teeth are found, incisors, canines, and molars,—at least there is no decided absence of either; for even when the laniar character is obscure, the transitional canines occupy that space which is left vacant in the Rodentia, or Glires. They have no horizontal motion of the jaw, their smell is great, and their intestines short. They are more or less exclusively carnivorous by nature, according to the relative developement of their normal structure, especially of the laniar teeth; from the lion and the tiger, which have them large and long, to the hedgehog, in which they are ambiguous. This last genus, as hath been already noticed, forms the connecting link between the Feræ and the Glires, as the Hyrax will connect the Unguipedates by the Glires, to the Cornipedates by the Bellusæ, as shall be seen hereafter. According to these circumstances, and the habits thereon dependent, the Feræ have been conveniently subsorted into three sections, the Sanguinariæ, the Predaceæ, and the Subterraneæ: the former

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two have also been termed Carnivoræ, to distinguish them from the latter, which are Insectivorous; and the Sanguinariæ have also been called Digitigrades, from their mode of walking, while the Predaceæ and Insectivoræ are Plantigrades. Indeed, by taking different points of structure and habit as criteria, almost as many distributions might be effected as there are organs in the animal frame; but such would be worse than useless labour; the only legitimate object being to contrast the advantages of the several plans, and select as characters those which are the most obvious, constant, and decisive; not alone because they are common, nor yet because they are original, but because they promise to be the most perspicuous and the most useful. Therefore all those unguipedate pouchless quadrupeds which have three sorts of teeth, whether more or less developed, and whatever be their number, are associated in one race: the Tridentulæ, Plenidentatæ, or Plenidentia of the dental scheme: the savage beasts of the vulgar; the FERÆ of Zoologists. As now enumerated, they are analogous to the Feræ of Linnæus, excluding Phoca and Didelphis; and to the Carnivora of Cuvier, excluding the Amphibia. (*Vide* Table, p. 349.)

The most distinguishing characteristic of the GLIRES, or Rodentia, the second race of unguipedate quadrupeds, is the universal absence of laniar teeth, instead of which is found the signal diastemama, unmarked by even transitional canines; the incisors, usually two in each jaw, are chisel-shaped, and so far protruded as to give these animals a peculiar facility in gnawing, whence one name; their molars are more or less complex and lamellate; in the frugivorous species the crowns are nearly flat; in others, whose food is of a more varied kind, they are tuberculated; in most the intestines are long and the cæcum voluminous. Their feet are little different from the Feræ, but their toes are rather armed with nails, than claws, which are unable to seize and unfitted to tear any living prey; the two bones of the forearm are often united, and can scarcely ever be regarded as having the power of turning, while the Feræ have a limited privilege both of pronation and supination; though considerably less than the monkey type. In like manner, as some of the Insectivorous Feræ have clavicles and others none, so is it with the Glires; and from this circumstance two

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or even three sections have been distinguished in this race, the first with clavicles, as the beaver, the rat, the squirrel, &c.; the others, with rudiments only, or without, as the porcupine, the hare, and the cavy. The synonymes, as analogous to the several titles of the preceding race, may be enumerated Didentulæ, from their two sorts of teeth; GLIRES (Liberæ), or free beasts, from their habits; and as now enumerated, they vary little from the Glires of Linnæus, the Rodentia of Blumenbach, the Rongeurs of Cuvier, the Prensiculantia of Illiger, the Celerigrades of Blainville, &c. &c. (*Vide* Table, p. 350.)

The Unguipedates hitherto unmentioned are chiefly characterized by negative qualities; in them even the digits become less and less in number, are less distinct and less exserted; the teeth are never of more than two kinds, usually canines and molars, the incisors are almost always absent; in some, as the Tatusia, and Orycteropus, there are molars only, and in others, even the fibrous molars are wanting, and there are no teeth at all. These animals not only connect the quadrupedous mammalia with the Monotremes, but also by some of their species, as the scaly ant-eaters, to the Chelonian reptiles. From their common want of incisor teeth, they have been called Exincisoria; from the absence of the canines, as well as from the imperfect structure of the molars when present, and the frequent absence of all, Edentulæ, Edentatæ, or Edentia; but, perhaps, from their habits their best name would be SUBFERINÆ, or Semiferæ, wild beasts; thus contrasting them with the Feræ and the Liberæ, the free and savage beasts. As now enumerated, they correspond to the Bruta of Linnæus, excluding the elephant and the walrus; the Effodientia and Tardigrada of Illiger, excluding Prochilus; and the Edentés of Cuvier, excluding the Monotremes. (*Vide* Table, p. 351.)

The PRÆCOCINATÆ (Præcocinates); Marsupiatæ (Marsupiates); Mastothecatæ (Mastothécates); sacculated or teat-pouched beasts, which form the second district of the type of Quadrupeds, strongly characterized by the extraordinary perversion of their genitals, were gathered together from the several orders into which their varied dental developements would disperse them, and associated most philosophically by Cuvier as a separate tribe; and although, in his arrangement, they form but a sub-section of the Carnassiers, this anomaly has

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been redressed by succeeding writers; for the Carnassiers are defined as possessing three sorts of teeth, while some of the Marsupials want canines in one, and some in both their jaws; being, in fact, sacculated glires: they constitute by this, their index, a series essentially distinct.

But few of these animals were known to Linnæus, and all that were then discovered he included in the single genus Didelphis, which was arranged among his Feræ; and both Blumenbach and Illiger adopted a nearly similar distribution. Did their number require a subordinate demarcation of the district into races, the variety of their dental developement would seem to furnish a convenient guide. Whether future discoveries may suggest more natural groups can only be matter of conjecture.

On the one hand, we find a race of these sacculated animals very rapacious, and possessed of three sorts of teeth, having canines in both their jaws; hence this race may be called the Feridentia: and, were it not for their pouches, they might be arranged with the Feræ, allying themselves to the Insectivora, being chiefly Entomophagous. In another race the upper canines are wanting; these are the Kangaroo Rats: they are chiefly carpophagous; and, from their dental structure, form a connexion between the Feræ and the Glires, as also between one section of the Marsupiates and the other: they may be called the Semidentatæ or Semidentia, and lead to the third Marsupial race, which have no canines at all in either jaw; thus associating with the Glires, and hence termed the Gliridentia. The Pedimanous species of this district would associate the Quadrupeds with the Manupeds; the Balantia shews an affinity to the Pleuropterus of the Alipeds; and the united digits of some other species would mark their affinity to the district which is next to follow. (*Vide* Table, p. 351.)

The CORNIPEDATÆ (Cornipedates), hoofed or hoof-footed quadrupeds, the last district of the present type, are characterized by having all their four extremities freely exserted by their feet terminating in toes more or less numerous and more or less developed; encased in hoofs, which are differently divided: none of them have abdominal pouches, and their dental system is very various. The three races are distinguished by having one, two, or more toes predominantly deve-

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loped, and inclosed respectively in whole, cloven, and subsulcate hoofs. The elephant, the hippopotamus, the rhinoceros, the hog, and their allies, will form the first race of the hoof-footed district: they are non-ruminant animals, with more than two toes inclosed in hoofs. Some difficulty would seem to have arisen as to the arrangement and the naming of this extraordinary race of quadrupeds. The elephant was placed among the Bruta of Linnæus, and associated with the walrus, the sloth, and the ant-eaters; his Bellusæ contained the hippopotamus, rhinoceros, hog, and horse. The association was much improved by Blumenbach, who excluded the Trichecus and the horse, and called the group Multungula. Cuvier, who retained the horse in this section, and added the Hyrax to the assemblage, called the whole, from the thickness of the skin in most, Pachydermata; but the Daman at any rate is not a Pachydermatous animal; and, as we exclude the horse, the name Bellusæ, which, in the classic writers, is more especially applied to uncouth, unwieldy beasts, seems the most fitting and expressive term. Had a word been to be invented, as the herds of swine and hordes of elephants are separated from the horse and the ruminating cattle, *i. e.* from the flocks, *Egreges* (out-herds) might not have been an inappropriate appellation for these Belluine beasts; its true meaning would point out their segregation from the flocks and herds; and its acquired signification would obliquely indicate their vast, unsightly forms. They might likewise be called Armenta or hordes; and, from their hoofs, Subungulosa, Multungula, Gravidada, &c. (*Vide* Table, p. 352.)

The Ruminating Quadrupeds, or second race of hoof-footed beasts, form so natural an assemblage, that all writers agree in their alliance, however they may differ in the names that are severally attached thereto: little comment will, therefore, here be needed. PECORA, Pecudines, or Cattle, would seem the most familiar and the best appellations: from their cloven hoofs they have been called Bisulca, Binungula, or Binungulosa; and Ruminantia, as they chew the cud. They have no true incisor teeth in the upper jaw, which, with their fourfold stomach, their ruminant propensity, and their cloven hoofs, will strongly characterize this natural group. The subdivisions of this race are formed by the presence or absence of canine teeth,

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the possession or non-possession of horns, or antlers, and their deciduous or persistent nature: but this is extraneous to our present purpose. The spurious canines of the camel, and its transitional hoofs, would seem to connect it with the other types, and form a link with the hoofed and the claw-footed beasts.—(*Vide* Table, p. 353.)

The horse and its allies, the hemione, the ass, the zebra, and the quaccha, the varieties of which are multitudinous, although they have mostly been referred to a single genus, and the chief of them even to a single species, will form the last race of the hoofed quadrupeds; the indicial character of which will be the possession of true incisor teeth in both jaws; each extremity terminates in a single greatly developed digit encased in an undivided hoof: the lateral toes may be seen as mere abortions. These animals do not ruminate. CABALLINÆ, Jumenta, Solipeda, Solidungula, Monungulosa, Exarmenta, are the various synonymes which different considerations might, with almost equal justice, apply to this race of beasts; the first is that which may be probably preferred. (*Vide* Table, p. 352.)

To pursue this analysis through all the kinds, the genera, and the species; or to critically examine how far many of the genera would admit, and, indeed, require subdivision, would far exceed the scope of the present essay; the following tables must now suffice: the developement of each kind would, in itself, form matter for an especial dissertation. All that was here proposed has been to indicate, and that in outline only, the general connexions of the kinds, the races, and the types of quadrupeds: and, as far as may be practicable, to render consentaneous popular and scientific terms. The uncouth and elaborate titles, with which too many genera and species are encumbered and disguised, might often in like manner be most advantageously reformed, and their magniloquent epithets be changed for other more simple and familiar names; but this would anticipate a future consideration, and involve a question of too great extent to be compatible with the present form and stage of these outline indications.

The genera of Linnæus, however, it may be remarked, here as elsewhere, are rather associations of secondary than of primary groups of species; they are, in fact, rather kinds than

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genera: *i. e.* rather a group of genera than a genus. His great ability and consummate skill were chiefly shewn in forming these general associations; and, in the ample range which his powerful mind took of the whole of nature, what wonder is it that, when much fewer individuals of each kind were known, many genera truly distinct were coalesced; and these sub-genera modern zoologists must, by degrees, distinguish and separate as such: yet, not to lose the advantage of his comprehensive scheme, retaining, as far as they are correct, his genera as the next larger groups or kinds. Thus his Canis, Felis, Ursus, Mus, &c. form our Canidæ, Felidæ, Ursidæ, Muridæ; and in like manner of the rest. Again, his genus Canis, which gives name to the kind, forms its sub-sections into genera, as Canis, the true dog; Vulpes, the fox, the hyæna, &c. Mus, also, which becomes Muridæ, gives as its sub-genera, now genera, Mus, the mouse and rat; Arctomys, the marmot; Cricetus, the hamster; Myoxus, the dormouse, &c. &c.

Type, QUADRUPEDA (Quadrupedes). Quadrupeds, truly four-footed beasts. Mammalious, ditrematous; furnished with four well-developed and freely-exserted limbs, terminating in feet.

Districts, UNGUIPEDATÆ (Digitate, or nailed, *i. e.* Unguipedate quadrupeds). Feet terminating in distinctly developed toes, or in digits less freely exserted, armed with nails: no abdominal pouches.

PRÆCOCINATÆ (Præcocinates, Marsupiatæ, Marsupiates, Mastothecatæ, Mastothecates, sacculate or teat-pouched beasts). Destitute of true placenta? The females parturient prematurely; generally furnished with a marsupium. Marsupial bones in both sexes.

CORNIPEDATÆ (Cornipedates, or hoofed quadrupeds). All the extremities terminating in toes more or less numerous, and more or less developed, but the digits enclosed in hoofs; no abdominal pouches.

UNGUIPEDATÆ,

Races, FERÆ, Plenidentia, or Tridentulæ; savage beasts. Three sorts of teeth—incisors, canines, and molars; more or less distinct; always in one, generally in both jaws.

GLIRES, Rodentia, or Bidentulæ, Liberæ; free beasts. Two sorts of teeth only, incisors and molars; never canines; a distinct diastemma in their stead: the incisors signally protruded, and chisel-shaped.

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SUBFERINÆ, Semiferæ, Edentia, Edentula, or Exincisoria; wild beasts. Teeth various, sometimes none; occasionally canines and molars, sometimes molars only, often none; almost never any incisor teeth; and the other teeth often without roots.

PRÆCOCINATÆ,

Races, Feridentiæ, Feridentulæ, Feridents; three sorts of teeth in each jaw: chiefly entomophagous.

Semidentiæ, Semidentulæ, Semidents; three sorts of teeth in the upper jaw; only two in the lower, the inferior canines being absent: chiefly carpophagous.

Gliridentiæ, Gliridentulæ, Glirideuts. Two sorts of teeth only; no canines in either jaw: chiefly phyllophagous.

CORNIPEDATÆ,

Races, BELLUINÆ, Egreges, Armenta, Multungula, Subungulosa; Belluines, Outherds, or Hordes. Three to five toes on each foot, more or less equally developed, and included in a subungulate horny case. Not ruminant. Teeth various.

PECORA, Bisulca, Binungula, Ruminantia; Cattle or Pecudines. Cloven hoofs; no true incisors in the upper jaw. Stomachs more or less numerous and complex: ruminant.

CABALLINÆ, Jumenta, Exarmenta, Solidungula, Solipeda, Monungulosa: Caballines, Cavalry, Stud, Troops. A single greatly developed digit, with lateral abortive toes. A single undivided hoof: true incisors in both jaws: not ruminant.

Type.	*Districts.*	*Races.*
QUADRUPEDA, QUADRUPEDS.	UNGUIPEDATÆ, UNGUIPEDATES.	FERÆ, Savage beasts.
		GLIRES, Free beasts.
		SUBFERINÆ, Wild beasts.
	PRÆCOCINATÆ, MARSUPIATÆ, MARSUPIATES, PRÆCOCINATES.	FERIDENTIÆ, Opossum race.
		SEMIDENTIÆ, Potorace.
		GLIRIDENTIÆ, Kangarace.
	CORNIPEDATÆ, CORNIPEDATES.	BELLUINÆ, Belluines.
		PECORA, Cattle.
		CABALLINÆ, Caballines.

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Race.	*Kinds.*	*Genera.*	*Species.*	
FERÆ PLENIDENTILÆ, Savage Beasts: Ferinæ.	Canidæ, Dog-kind.	Canis	Familiaris,	Dog.
		? Lupus	— —	Wolf.
		Vulpes	Communis,	Fox.
		— —	Lagopus,	Arctic Fox.
		Fennecus	Brucei,	Fenneck.
		Lycaon	Tricolor,	Lycaon.
		Hyæna	Striata,	Striped Hyena.
		— —	Crocuta,	Spotted Hyena.
	Felidæ, Cat-kind.	Acinonyx	Venator,	Guepard.
		Felis	Tigris,	Tiger.
		— —	Pardalis,	Panther.
		— —	Catus,	Cat.
		? Leo	Africanus,	Lion, African.
		— —	Asiaticus,	Lion, Asiatic.
		Lynceus	Chaus,	Booted Lynx.
		— —	Caracal,	C. Lynx.
	Mustelidæ, Weasel-kind.	Putorius	Fœtidus,	Fitchet.
		— —	Vulgaris,	Weasel.
		Mustela	Furo,	Ferret.
		— —	Foina,	Marten.
		— —	Zibellina,	Sable.
		Mephitis	Striata.	Skunk.
		Lutra	Fluviatilis,	Otter.
	Viverridæ, Civet-kind.	Viverra	Civetta,	Civet.
		— —	Zibetha,	Zibeth.
		Suricata	Capensis,	Cape Sur.
		Genetta	Communis,	Common Genet.
		Herpestes	Ichneumon,	Egyptian Ichneumon.
		— —	Mungos,	Indian Ichneumon.
	Ursidæ, Bear-kind.	Gulo	Arcticus,	Wolverene.
		— —	Larvalis,	Masked Glutton.
		Meles	Taxus,	Badger.
		Ursus	Arctos,	Brown Bear.
		— —	Americanus,	Black Bear.
		— —	Maritimus,	White Bear.
		Prochilus	Hirsutus,	Shaggy Prochil.
		Procyon	Lotor,	Racoon.
		Nasua	Rufa,	Red Coati.
		— —	Fusca,	Coati Mondi.
	Talpadæ, Mole-kind, or Soricidæ, Shrew-kind.	Talpa	Europæa,	Common Mole.
		Chrysochloris	Capensis,	Cape Mole.
		— —	Rufa,	Red Mole.
		Condylura	Radiata,	Rayed.
		Tapaia	Tana,	Tapaia.
		Scalops	Canadensis,	Canad. S.
		Sorex	Araneus,	Shrew-fœtid.
		— —	Constrictus,	
		— —	Lineatus,	
		Mygale	Moscovitica,	Musk Shrew.
		— —	Pyrenaica,	Pyrenean.
	Erinacidæ, Urchin-kind.	Centenes	Semispinosus,	Tenrec.
		— —	Setosus,	Bristly C.
		— —	Spinosus,	Spiny Tendrac.
		Erinaceus	Europæus,	Urchin, or Hedgehog.
		— —	Malaccencis,	Malacca.
		— —	Auritus,	Long-eared.

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Race.	*Kinds.*	*Genera.*	*Species.*	
GLIRES, RODENTIA, LIBERÆ: Gnawing beasts, Glirines.	Hystricidæ, Porcupine-kind.	Hystrix	Cristata,	Crested P.
		— —	Macroura,	Rice-tailed.
		Erithizon	Dorsatus,	Erithizon.
		Sphiggurus	Villosus,	Hairy Sphig.
		— —	Spinosus,	Spiny Sphig.
		Sinastherus	Cuandu,	Cuan.
	Leporidæ, Hare-kind.	Lepus	Timidus,	Hare.
		— —	Glacialis,	Snowy Hare.
		— —	Cuniculus,	Rabbit.
		Lagomys	Alpinus,	Pika.
		— —	Pusillus,	Calling Hare.
		— —	Ogotona,	Ogotone.
	Caviadæ, Cavy-kind.	Hydrochærus	Capybara,	Capyb.
		Cælogenus	Fulvus,	Paca.
		— —	Subniger,	Brown Paca.
		Cavia	Cobaya,	Cavy.
		Dasyprocta	Acuti,	Agouti.
		— —	Acuschy,	Akouchy.
	Sciuridæ, Squirrel-kind.	Sciurus	Vulgaris,	Common Squirrel.
		— —	Cinereus,	Grey Squirrel.
		Cheiromys	Madagascarensis,	Aye Aye.
		Sciuropterus	Volucella,	Polatouche.
		Pteromys	Petaurista,	Taguan.
	Muridæ, Mouse-kind. or Rattidæ, Rat-kind.	Sphermophilus	Citillus,	Souslik.
		Arctomys	Alpinus,	Alp. Marmot.
		Pedetes	Capensis,	Cape Pedetes.
		Bathyergus	Capensis,	Cape Bathyerg.
		Dipus	Sagitta,	Jerboa.
		— —	Jaculus,	Siberean.
		Gerbillus	Indicus,	Indian Gerb.
		Aspalax	Typhlus,	Blind Zemni.
		Cricetus	Vulgaris,	Common Hamster.
		— —	Migratorius,	Wandering.
		Mus	Rattus,	Black Rat.
		— —	Decumanus,	Norway Rat.
		— —	Musculus,	Common Mouse.
		— —	Sylvaticus,	Field Mouse.
		— —	Minutus,	Tiny Mouse.
		Hydromys	Chrysogaster,	Yellow-bellied H.
		— —	Coypus,	Chilese.
		Myoxus	Avellanarius,	Common Dormouse
		— —	Nitella,	Garden Lerot.
		— —	Glis,	Fat Loir.
		Echimys	Cristatus,	Crested E.
		Lemmus	Terrestris,	Lemming.
		— —	Norvegicus,	Norway L.
		Arvicola	Arvalis,	Field Arvicole.
		— —	Amphibia,	Water Rat.
	Castoridæ, Beaver-kind.	Ondatra	Zibethica,	Musquash.
		Castor	Fiber,	Beaver.

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Race.	*Kinds.*	*Genera.*	*Species.*	
SUBFERINÆ EDENTIA, Subferines: Wild Beasts.	Achedæ, Sloth-kind.	Unaus	Didactylus,	Unau Sloth.
		Acheus	Tridactylus,	Ai Sloth.
		Megatherium	— —	Fossil Sloth.
	Tatusidæ, Tatu-kind.	Daysypus	Apar,	Apara.
		Tatusia	Peba,	Peba Tatu.
		— —	Hybrida,	Armadillo mule.
		Priodontes	Giganteus,	Giant Tatouhou.
		Chlamyphorus	Truncatus,	Mantled Tatouhou.
		Orycteropus	Capensis,	Cape Oryc.
	Manidæ, Manis-kind, or Ant-eaters.	Myrmecophaga	Jubata,	Tamanoir.
		— —	Tamandua,	Tamandua.
		Manis	Brachyura,	Short-tailed Pangolin.
		— —	Macroura,	Long-tailed Pangolin.
		— —	Javanicus,	Javanese.

Dist.	*Races.*	*Kinds.*	*Genera.*	*Species.*	
PRÆCOCINATES. Marsupiatæ.	Ferulentiæ, Opossum-race.	Didelphidæ,	Didelphis	Virginiana,	Virg. Opossum
			— —	Cancrivora,	Crab-eating
			— —	Opossum,	Carigueia
			Monodelphis?	Dorsigerens,	Touan
			— —	Brachyura,	Short-tailed T.
			Cheironectes	Palmata,	Yapock.
		Dasyuridæ,	Dasyurus	Ursinus,	Ursine or native Devil
			— —	Macrourus,	Long-tailed
			Thylacynus	Striatus,	Zebra-Thylacyne
			Phascogale	Penicillatus,	Brush-tailed P.
			— —	Minimus,	Little
			Perameles	Nasuta,	Long-nosed.
		Phalangistidæ,	Phalangista	Maculata,	Spotted P.
			— —	Cookii,	Cook's
			Balantia	Lemurina,	Lemurine
			Petaurista	Sciurea,	Squirrel-like
			Acrobata	Pygmæa,	Dwarf.
PRÆCOCINATÆ. Explacentaria.	Semiferentiæ, Koala-race.	Koalidæ,	Potorus	Murinus,	Potoroo
			— —	Peronii,	Peron's
			Koala	Subiens,	Climbing K.
	Elliferentiæ, Kangaroo-race.	Macropidæ,	Kangurus	Labiatus,	Larger Kang.
			— —	Fuliginosus,	Sooty Kangaroo.
			— —	Rufus,	Red Kangaroo.
			Macropus	Ulabattus,	Bush Kangaroo.
			Halmaturus	Elegans,	Elegant Kang.
		Vombatidæ,	Phascolomys	Vombatus,	Wombat.

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CORNIPEDATÆ, UNGULOSA: Cornipedate, hoofed or hoof-footed Quadrupeds.

Races.	*Kinds.*	*Genera.*	*Species.*	
Belluinæ, Belluines, Egreges, Hordes.	Elephantidæ, Elephant-kind.	Elephas	Indicus,	Indian Elephant.
		— —	Africanus,	African.
		Mammuthus	Borealis,	Fossil Mammoth.
		— —	Meridionalis,	
		Mastodon	Giganteum,	Gigantic.
		— —	Angustidens,	Lesser.
	Hippopotamidæ, Hippopotamus-kind.	Hippopotamus	Capensis,	C. Hippo.
		— —	Senegalensis,	S. Behemoth.
		— —	Antiquus,	Fossil.
	Rhinocerosidæ, Rhinoceros-kind.	Rhinoceros	Indicus,	Indian R.
		— —	Sondaicus,	Sondic.
		— —	Sumatrensis,	Sumatra.
		— —	Africanus,	African.
		Elasmotherium	Fischerii,	
	Tapiridæ, Tapir-kind.	Tapirus	Americanus,	Am. Tapir.
		— —	Malayanus,	Malayan T.
		Lophiodon	Tapiroides,	Fossil.
		Palæotherium	Cuvierianum,	Fossil.
	Suidæ, Pig-kind or Swine.	Sus	Aper,	Wild Boar.
		— —	Scrofa,	Domestic Hog.
		— —	Priscus,	Fossil.
		— —	Larvatus,	Masked Boar.
		— —	Indicus,	Indian.
		Phascochœrus	Æthiopicus,	Æthiopian.
		Babyrussa	Indica,	Indian B.
		Dicotyles	Torquatus,	Patira or Peccary.
		— —	Labiatus,	Tajassu.
Caballinæ, Caballines, Cavalry or Stud.	Equidæ, Horse-kind.	Asinus	Vulgaris,	Common Ass.
		— —	Zebra,	Zebra.
		— —	Quaccha,	Quagga.
		— —	Montanus,	Dauw.
		Equus	Hemionus,	Hemione or Dziggtai
		— —	Caballus,	Horse.

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CORNIPEDATÆ, UNGULOSA:—(*continued.*)
Cornipedates, hoofed or hoof-footed Quadrupeds.

Races.	*Kinds.*	*Genera.*	*Species.*	
Pecora, Pecorines, Cattle, Flocks.	Camelidæ, Camel-kind.	Camelus	Bactrianus,	Camel.
		— —	Dromedarius,	Dromedary.
		Auchenia	Vicunna,	Paco.
		— —	Llacma.	Lama.
	Moschidæ, Mosch-kind.	Moschus	Moschiferus,	Musk Deer.
		— —	Memina,	Memina.
		— —	Javanicus,	Javanese.
	Cervidæ, Deer-kind.	Cervus	Elaphus,	Stag.
		— —	Canadensis,	Wapiti.
		Alces	Europæus,	Elk, or Moose Deer.
		— —	Coronatus,	Crowned.
		Tarandus	Rangifer,	Rein Deer.
		Dama	Vulgaris,	Common Hart,
		— —	Maculata,	or Fallow Deer.
		Rusa	Hippelaphus,	Great Rusa.
		Capreolus	Pygargus,	Ahu.
		— —	Dorcas,	Roebuck.
	Giraffidæ, Giraffa-kind.	Camelopardalis	Giraffa,	Giraffe.
	Antilopidæ, Antilope-kind.	Antilope	Cervicapra,	Common A.
		Gazella	Dama,	Swift Gazelle.
		— —	Pygarga,	White-faced.
		Tragelaphus	Sylvaticus,	Boschbock.
		Antilocapra	Furcifer,	Prong-horned A.
		Rupicapra	Hamulicornis,	Chamois.
	Bovidæ, Ox-kind.	Boselaphus	Pictus,	Nylghau.
		Catablepas	Gnu,	Gnoo.
		— —	Taurina,	Kokoon.
		Bos	Bubalus,	Buffalo.
		— —	Taurus,	Beeve.
		— —	Grunniens,	Yak.
		Ovibos	Moschatus,	Musk-Ox.
		— —	Pallantis,	Fossil-Ox.
	Hircidæ, Goat-kind.	Capra	Hircus,	Goat.
		— —	Ægagrus,	Wild G.
		— —	Ibex,	Ibex.
	Ovidæ, Sheep-kind.	Ovis	Tragelaphus,	Bearded Sheep.
		— —	Ammon,	Argali.
		— —	Montana,	American Arg.
		— —	Aries,	Common Sheep.

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Chemical Observations. By Thomas Graham, A.M., F.R.S.E., Lecturer on Chemistry, Glasgow.

1. On the Application of spongy Platinum to Eudiometry.

In explaining the action of cold spongy platinum in disposing the union of mixed oxygen and hydrogen gases, it seems necessary to suppose that hydrogen is really accendible at common temperatures, but that its point of accension is unnaturally elevated in ordinary circumstances when it is not in contact with highly-divided matter, just as the boiling point of water and other liquids is elevated in smooth glass vessels. This view may be correct, although it only shifts the difficulty; for we have still to explain why hydrogen, if so accendible, does *not* take fire at superior temperatures when out of contact with minutely divided matter. But there is an apparent analogy between the circumstances of the suspension of the combustion in the one case, and of the ebullition in the other, on which the mind can rest with some satisfaction.

Soon after the discovery of Dobereiner, it occurred to both Dr. Henry and Dr. Turner to apply the principle to the analysis of mixed gases. But they immediately found that hydrogen could not at all times be withdrawn from a gaseous mixture by the action of spongy platinum, although the required addition of oxygen was made, as the action of that substance was paralysed or entirely suspended by the presence of certain gases in the mixture. The following table of Dr. Henry's exhibits the sum of our information on this subject.

"The first column exhibits the number of volumes of each gas required to render one volume of an explosive mixture of hydrogen and oxygen (in the usual proportion of two hydrogen to one oxygen) uninflammable by the discharge of a Leyden jar; while the second column shews the number of volumes of each gas necessary, in some cases, to render one volume of an explosive mixture insensible to the action of the sponge, and in other cases indicates the number which may be added without preventing immediate combination.

"In the first column, the numbers marked with an asterisk were determined by *Sir H. Davy*: the remaining numbers in

that column, and the whole of the second, are derived from my own experiments :—

One volume of explosive mixture was rendered incapable of being inflamed by electricity when mixed with			Effect of adding the same gases to one volume of explosive mixture on the action of the sponge.
About	*8 vols	of hydrogen - -	not prevented by many vols.
,,	6 ,,	nitrogen - -	ditto.
,,	*9 ,,	oxygen - - -	not prevented by 10 vols.
,,	*11 ,,	nitrous oxide -	ditto.
,,	1.5 ,,	cyanogen - -	prevented by 1 vol.
,,	*1 ,,	carbonized hydr.	not prevented by 10 vols.
,,	4 ,,	carbonic oxide -	prevented by ½ vol.
,,	*0.5 ,,	olefiant gas - -	prevented by 1.5 vol.
,,	*2 ,,	muriatic acid -	not prevented by 6 vols.
,,	2 ,,	ammoniacal -	not prevented by 10 vols.
,,	3 ,,	carbonic acid -	ditto."

From other observations of Dr. Henry, carbonic oxide appears to retard the combustion of hydrogen, by taking the precedence of the latter in uniting with oxygen, from superior inflammability. This I found to be much more strikingly the case with sulphuretted hydrogen gas, which Dr. Turner found to suspend the action of the sponge, when present even in the most minute proportion. When mixed with oxygen, this gas slowly disappeared under the influence of a dried clay pellet, containing spongy platinum; the hydrogen only uniting with the oxygen, and the sulphur being deposited in the ball, which was soon thereby rendered inactive, but not till it had destroyed two or three hundred times its bulk of sulphuretted hydrogen in the course of twenty-four hours. In a mixture of sulphuretted hydrogen, hydrogen and oxygen gases in equal volumes, the oxygen united in twenty-fours to the hydrogen of the sulphuretted hydrogen, nearly to the entire exclusion of the free hydrogen; but the union of the last with the remaining oxygen was determined in a few seconds by throwing up a fresh platinum ball.

Sulphurous acid gas is as efficient in this way as sulphuretted hydrogen (Turner); yet, upon trial, the sponge had no effect in determining the union of oxygen and sulphurous acid, even with the presence of moisture.

Olefiant gas in my hands was at first as powerful in preventing the combustion of explosive mixture as it was found to be by

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both Henry and Turner; but on washing that gas more sedulously with caustic potash, its interference was found to depend on a trace of impurities, for the ball always acted on explosive mixture within a few minutes, however largely diluted with this gas, if properly purified. Indeed, I had frequent occasion to separate hydrogen from olefiant gas, and found cold spongy platinum most effectual for the purpose. Neither did sulphuric acid vapour retard the action of the platinum; indeed, the other allowed the action to go on so rapidly, that from the elevation of temperature it was itself slightly acted upon, carbonic acid always appearing. The same was the case with the vapours of naphtha and the essential oils.

The action of these gases here, therefore, is unlike the action of the same gases and vapours in protecting phosphorus from oxidation.

The influence of sulphurous acid and sulphuretted hydrogen is not impaired by diminishing the barometric pressure.

2. Crystallization of Barley-Sugar.

The change in appearance, arising from crystallization, which sticks of barley-sugar undergo in keeping, is always instanced as a case of crystallization occurring in a solid body, without solution, and independently of external agents. The barley-sugar certainly does not then become a hydrate; and probably at the completion of the change is exactly of the same weight as before it began. But from an observation I have made, it would appear that the presence of a little moisture is necessary for the change, and probable that every portion of barley-sugar which suffers this change has been successively loosened and held in solution by that small portion of water, which begins to act on the outer surface of the stick and travels inwards.

Two fresh sticks of barley-sugar, dry and transparent, were introduced at the same time into separate phials; one of them with a stick of caustic potash, and the other by itself, corked up, and laid in a drawer. The barley-sugar, in company with the caustic potash, which would preserve it perfectly dry, did not undergo the slightest alteration in six months, but remained as transparent as at first. The barley-sugar in

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the other phial was scarcely altered during the first four months; but during the last two months, which were colder and damper, it became opaque on the surface, and the crystallization thereafter was propagated inwards to a considerable depth.

The effect of a small quantity of moisture in enabling solid amorphous matter to crystallize was observed very distinctly in the case of another substance. A quantity of sulphate of soda was rendered anhydrous by heat, and became a heavy powder. Placed in a confined atmosphere, kept purposely humid, the powder slaked like lime, swelling to several times its original bulk. It regains in two weeks its usual combined water (twelve atoms), and was then dry, and not in the slightest degree crystalline. Two days afterwards the powder was found a mass of crystals of the usual form of sulphate of soda, so dry as not to adhere to the blade of a knife; and it was not till after weighing that I satisfied myself of the presence of uncombined moisture among the crystals nearly to the extent of an additional atom. Here a small quantity of water allowed the powdery particles to right themselves, and adopt a crystalline arrangement, which they were incapable of assuming without it.

3.—Detection of Arsenic.

Perhaps no greater degree of certainty is desirable in the recognition of arsenious poisons than is attained by the reduction to the metallic state with subsequent oxidation; and certainly the addition to the usual suite of operations, which is the object of this notice, did appear, when first suggested to me by Mr. Clark of Glasgow, as a thing worth trying, an over-refinement, should it be found to be practicable. But, on ascertaining the extreme facility of the proposed process by actual trial, a view of the absolute certainty of the demonstration which it clenches completely altered my first opinion.

We usually stop on recovering the arsenious acid in minute crystals in the upper part of the tube. Scratch the glass tube with a file below the crystalline crust, and break it off. The upper part of the tube containing the supposed arsenious acid, and nothing else, may be boiled for a few seconds with a drachm or two of distilled water, to which a drop of caustic ammonia has been added. A solution is obtained, which, after being

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acidulated in the slightest degree by pure nitric or acetic acid, and divided into several portions on watch-glasses, may yield the most distinct and characteristic indications with the three best liquid tests, ammonio-nitrate of silver, ammonio-sulphate of copper, and hydro-sulphuret of ammonia.

The best proof I can give that there is no difficulty in this application of liquid tests is, that it has succeeded with every one of my practical pupils, setting out with the reduction of a quantity of the sulphuret of arsenic which never exceeded one-tenth of a grain, although it was in general their first attempt to test for arsenic.

The process for demonstrating the presence of arsenic seems, therefore, to be finally perfected by this unexceptionable application of the liquid tests, devised by Mr. Clark.

4.—Chrome Orange.

It is singular, that, although no other colour has been so much run upon for a couple of years in cotton yarns, no account of the mode of raising this beautiful tint, so far as I can learn, has hitherto been published; yet the process is universally known, and followed by dyers.

The first object is to procure upon the yarns a good body of chrome yellow, of the ordinary and familiar tint of chromate of lead. For this purpose, the goods are well charged with protoxide of lead, which is done by dipping them in solution of acetate of lead, and then decomposing the salt by lime-water, of which the lime takes the acetic acid, and leaves the oxide of lead in the cloth. Every trace of lime must then be got rid of by washing.

It is necessary to have nothing but oxide of lead in the cloth; for, with acetate or nitrate of lead as the mordant, the colour will be uneven. The goods are then passed through a bath of bichromate of potash, which instantly strikes the chrome yellow with the oxide of lead.

The orange is raised by throwing the goods so prepared into lime-water at or near a boiling heat. Lime, at that temperature, appears to be capable of partially decomposing the chromate of lead, taking half the chromic acid from a greater or less portion of that salt, and reducing it to the state of dichromate of lead.

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The dichromate of lead is itself of a full red colour, and is best prepared according to the original process of Mr. Badam's (*Annals of Philosophy*, N. S. vol. ix.), by digesting a solution of the yellow chromate of potash upon carbonate of lead at a boiling temperature, in the proportion of one atom of the former to two atoms of the latter, stirring up the solid matter very frequently, as the action is far from energetic.

Caustic potash likewise converts the chromate of lead into the dichromate; but, from its strong disposition to dissolve the oxide of lead, as well as to withdraw chromic acid, did not answer with us for forming the dichromate.

It occurred to Mr. R. Ruathen, while engaged with this subject in my laboratory, to try if the dichromate would stand the glazing heat of a potter's kiln, which it was found to do, and to form a pretty good red on ordinary kinds of stone-ware.

Geological Survey of the Island of Jersey. By Lieutenant Nelson, *Royal Engineers, Corresponding Member of the Plymouth Institution.**

The Jersey rocks may be referred to three classes, that of the argillaceous schists, that of such as are distinguished by the presence of felspar, and lastly, that of the breccia, composed of both.

The felspar rocks overlay the breccia and argillaceous schist, alternating with the last, which is also overlaid by the breccia.

Argillaceous Schist.

Beginning at L'Etac in St. Ouen's bay, a remarkable junction is observable in the little bay *a*, where the sienite joins the schist, and penetrates it in large irregular veins. About a mile to the south is the mouth of a remarkable valley, at the bottom of which, for nearly its whole length, lies the line of separation between argillaceous schist and the sienite, which last is obviously incumbent: a small portion of the former

* The letters in this paper refer to a map, which we have not been able to prepare for insertion in this Number.

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crosses the mouth at *b*, and comes sharply to the foot of the latter, without the slightest appearance of intermixture. Proceeding south-east from the head of the valley to the point *c*, where the two main branches of the St. Mary's valley separate, another spot of junction is obtained, the rocks observing the same order. *d* is in the St. Laurence valley, at the reservoir for the mill, one mile south of St. John's. From *d*, going south and passing *e*, you arrive at the Town Mills *f*, in the St. Helier's valley; at about 60 yards from the mill, the green porphyry overlies the argillaceous schist, in the most decided manner; this last re-appears at *g*, on the south side of a slight hollow, close to Mr. Ingouville's, and so continues until it disappears under a dark greenstone at *h*; on the eastern side of the hill it buries itself finally under the marshes. At Gallow's Hill, just where the St. John's road ascends the brow at *i*, it is distinctly seen underlaying the green porphyry, as this last stretches out towards Elizabeth Castle. Coasting St. Aubin's Bay, until you arrive at the valley *i'*, to the south of, and immediately underneath the St. Aubin's signal staff, you proceed northward until you reach the elbow *j* of the St. Aubin's valley, the stream of which crosses the line of separation of the argillaceous schist and sienite, in two or three places; the last point where I could trace the junction between the two rocks is at *k*, about 100 yards below the New-road Bridge (Pont Marqué). The two rocks can now still be traced to within a short distance of each other until you reach *l*, the last appearance of the sienite. The sands of the Quenvais render the line from *k* to *l* somewhat indistinct. Though the slate underlies the sienite, it frequently pervades the latter in conformable beds, particularly observable on the north-west coast; at Portelet bay; at the southern extremity of Fort Regent; and, above all, at Gouray, where the slate bed is, at least, 100 feet in breadth; small isolated portions of the schist are also often found completely detached, although in the vicinity. There are fifteen such beds between *i* and Portelet bay, varying from about 2 feet in breadth to 30 feet, and afford a complete example of the slides and faults in the mining districts; the beds running about east and west, and the latter north and south.

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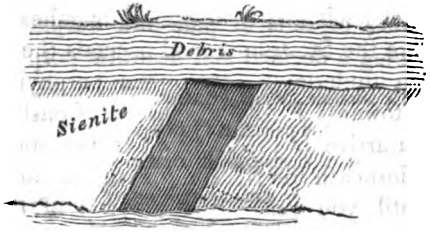
The comparative durability of the substances of the two rocks is singularly shewn by the invariable failure of the argillaceous schist before the action of the sea, having chasms and channels often of considerable depth; as is the case at the little insulated headland *n*, in Portelet bay, whereby a broad and deep dyke has been formed: the vein has also forked down at this point. At *o*, there is another similar chasm, at least 50 feet deep, and 20 wide; the walls of sienite are quite vertical.

So complete has been the removal of the argillaceous schist in these cases, that caverns 20 or 30 feet deep have been worn out in the cliffs where these veins first emerge; the



ceiling of these is actually composed of the debris of the vallies above, shewing clearly a more daring hand than that of man in their excavation.

The angle of inclination of the strata varies from 15° to 60°, dipping generally west of north.

21* exemplifies the nature of the rock from the centre, eastward, and 22, 22 *a*, 22 *b*, 23 from the centre, westward; these specimens present all the prominent points of character in the argillaceous schist, as it changes from a mere shale to the subcrystalline granular and extreme variety (found mostly near St. Aubin's) that verges closely on grauwacké. I have, also, met with small specimens of the roofing slate at Greve-de-Lecq. In the St. Peter's valley, the new road, winding along the bottom, finely exhibits the structure of this rock, as it crosses the strata in every direction.

That of which 21 is a specimen, crumbles and shales away

* The numbers refer to specimens sent by the author to the Museum of the Plymouth Institution.

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completely, from the action of a few years' sun and rain, (for there is but little frost in Jersey). This effect has been produced in a striking manner at *i*, on the north side of the St. John's road (see 24 and 25) in less than six years. Thin seams of black oxide of manganese, in *great abundance*, pervade the interstices between the planes of stratification.

Water is plentiful and excellent during summer and winter.

Felspar Rocks.

Under this head may be arranged felspar alone.

1. A very compact and intensely hard sort of a pink-grey colour, passing into

36. Another, equally hard, of a blue-green cast, but containing crystals of pale-buff felspar in abundance, forming a decided and beautiful porphyry.

26. 27. 28. A dark bluish green (externally, but dark grass green when cut and polished) porphyry, with the crystallized particles of a spicular form.

33. A light yellow ochre-coloured variety of No. 1, but more decidedly porphyritic.

10. An irregularly laminated and pink-red porphyry.

2. 4. 12. Ditto, containing concretions, sometimes nearly spherical (like those in the pisolite), in concentric coats, and internally radiated, at other times irregular; a curious rock.

3. A fine grained variety of 10.

5. A white variety of 2.

Felspar and Quartz.

43. A fine granular species.

Felspar and Hornblende (Greenstone).

15—39. 18—40. 19—44. Greenish-white felspar and hornblende in various proportions.

16 and 17. *Felspar* and *Schorl.*

13, 41, 42, 45, 52. *Felspar*, *Quartz*, and *Hornblende* (Felspar, red).

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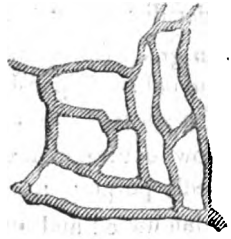
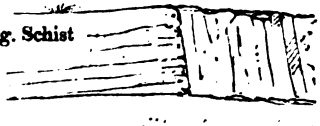
14. *Felspar, Quartz,* and *Green Mica* (Felspar, red).
50, 51, 53. *Felspar, Quartz,* and *Common Mica,* ditto.

FELSPAR ALONE.

1. This rock forms the sharp, prominent, and weather-worn columnar peaks along the coast from Tremont to the point *p* in Boluey bay, where it comes in contact with the breccia: in the same manner it runs from *r* to *s*, until it is overlaid by the sienite at Mont Orgueil. It seems to pass into two rocks; at *t* (in the Trinity valley) it forms peaks to 36; and all round the coast, as above pointed out, it invariably indicates 2, 3, and 10 below. Where exposed to the weather, its external surface, for about an inch in depth, sometimes breaks and cracks into facets, but without becoming soft.



26, 27, 28. I can only find these in the immediate neighbourhood of the argillaceous schist, as at Gallows Hill, round St. Saviour's, and at Gouray, accompanying the large bed*. It is the only one of the felspar rocks that in anywise intermixes with this last, which in this case assumes the form of a very compact writing slate, as a matrix containing dispersed but fairly mixed portions of the green specular porphyry. Its relative position, with respect to the argillaceous schist, as distinct from it, on the whole is shewn very decidedly at *f*, near the Town Mills.



It is a very imperfectly stratified rock, not homogeneous in

* Also in a vein of argillaceous schist near Noir Mont.

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composition, being, for the most part, full of cavities, which contain pulverulent black oxide of manganese, 32. This mineral penetrates it in every direction, giving it, when acted upon by the weather, a constant tendency to separate into small shapeless pieces.

Carbonate of lime (nearly the only hitherto observed appearance of it in the island) is also found in this rock in small and irregular portions. 29, 30, and 31, shew the green porphyry in a state of incipient decomposition, after an exposure of a few years. 37 is the same rock, containing iron pyrites, in the neighbourhood of a mineral water, *z*, in St. Saviour's parish.

At *x* and *y* it passes into 36 and 33 respectively.

36 is found at *u* in the Trinity Valley, at V in Anne Port, at Bonne-nuit bay, and Havre Giffard.

33. A very abundant rock extending over the yellow space, dashed with yellow: it decomposes, especially when just under the surface of the ground, as in specimens 34 and 35. It often presents a white striated appearance when superficially decomposed, thereby shewing a near relationship to 10.

10. The structure of this rock is best exposed on the sea-coast, where it appears as waved and striped, as if it had, in its soft state, been somewhat irregularly *worked* about.

It is pervaded by a large bed of argillaceous schist at Havre Giffard, and at the bay east of Belle Hougue, marked *y* (No. 11). When underground, but near the surface, the moisture decomposes it into its constituent and irregular laminæ, which crumble with great ease at the touch, and which become white when exposed to the sun in this state.

A reniform variety of felspar, with concentric coatings (No. 12), is found in small irregular masses among the hollows of this rock in St. Catherine's Bay.

3 and 2. From Boulay Bay, where they abound.

4. I am uncertain as to the absolute locality of this rock in

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this island, although it is found at Guernsey. Hitherto I have been unsuccessful in obtaining it *in situ;* but, from the large masses near Vicart and at Boulay Bay, the parent rock can be at no great distance.

These large masses are agglomerates of the same bodies, only they are about the size and shape of potatoes of various dimensions, standing out in full relief, of the colour of 12, which appears to be only a variety of this rock, the chief difference being the regularity of the concretions; which last circumstance, I have however found in every degree, assuming Nos. 4 and 12 as extremes. It is used in the construction of piers, and in cases where large blocks can be best turned to account.

5. Apparently a white variety of No. 2, which it underlies, and of which it contains small portions. The concretions shew evident symptoms of 5, *a*.

Epidote, serpentine, and diallage are found in small quantities at Boulay Bay. At the mouth of the Trinity Valley, the bed of the stream contains small rounded pebbles, covered with a coat of the black oxide of manganese, nearly one-fortieth of an inch thick.

Felspar and Quartz.

43.—From the neighbourhood of Noirmont and Le Corbière, where it is to be found in plenty.

Felspar and Hornblende.

15 is from La Motte, and not very abundant; it is taken from among 16 and 17 rocks; 18 and 19 are specimens of the whole coast of sienite, at the south-east of the island, beginning at Fort Regent, and reaching to Roque Berd; also from Sorel to the bay marked *x*, at the north of the island. This variety always assumes a reddish aspect (from the oxide of iron in the hornblende) at the surface; and the crust thus formed can be generally detached by a light and sharp blow. From the frequent inequality of disposition of the hornblende, this rock bears often a singularly mottled appearance, as at Elizabeth Castle, where the rock 38 may be well taken for a conglomerate, until this habit is known. Another example of this is given in 52. At the south-west point of the Hermit

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Rock (Elizabeth Castle), this alternate variegation of the red felspar, with the very compact and dark-coloured greenstone, is disposed nearly in horizontal layers, while the regular strata of the rock stand at an angle of 70 or 80 degrees; the red layers *always* projecting from the weather-worn failure of those in which the hornblende predominates. (See *fig.* D, p. 374.)

In figures A, B, C, and from the circumstance of the red veins very generally constituting the matrix of the green, and the substance of the veins that traverse it, as well as that in figure C; the green portions are of various descriptions, it may be questioned whether the red and green sienite are absolutely coeval.

49 is a good specimen of the very compact dark greenstone just alluded to, although it was taken from a large vein between Grosnez and Plemon. This last is not quite horizontal, but continues about 7 feet thick, whilst it gradually rises along the face of the almost vertically stratified cliffs: upwards of 3 feet in depth have been decomposed and disappeared. This vein divides in one place, which reduces the thickness of the two parts to that of about 3 feet each, until they rejoin and proceed as before. The same substance frequently accompanies the veins of argillaceous schist.

40, from Elizabeth Castle, as in 39; which last, although but an accidental stone from the beach, corresponds to 44, which was taken from a vein from Portlet Bay; it is, I believe, the hardest rock in the island.

20, shews the ultimate state of decomposition of the hornblende and felspar.

An east and west vein of chlorite mica; traverses the hill where Fort Regent stands, 19 *a*.

Felspar and Schorl.

16 and 17, From La Motte; not much of it. I have seen specimens in which the schorl lay as minute, confusedly scattered and divergent crystals in the felspar.

Felspar, Quartz and Hornblende.

The felspar in this variety is red, and this association with quartz and hornblende, forms by far the most extensive rock

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in the island. In the south-east it lies within the space marked out by Mount Orgueil, Granville church, *a'*, and La Hougue Bie. In the south-west there is hardly any other rock (except 43), and it is generally of a large, coarse-grained description.

The well-known Mount Madou, quarry 52, is the last point at which this sienite shews itself, where it overlays the pink-red porphyry, as decidedly as it does at Mount Orgueil. It reaches also from l'Etac to half-way between Greve-de-Leiq and Plemont, and from X Bay to Mount Madou. At X Bay all the three constituents are in large grains, which apparently present the most decomposable description of sienite. I have seen the hornblende fairly washed out by the sea, leaving a frail honey-combed skeleton of quartz and felspar.

Although this rock alternates with the argillaceous schist, yet there appears the utmost jealousy in keeping itself quite distinct; 46 was chipped from the junction at *a*, (l'Etac). (See *Fig.* E, p. 39). 47 shews the quartzose nature of it as as composing the vein, and which becomes gradually though decidedly sienite at about 20 feet from the point where it enters the argillaceous schist. At *a* are numerous veins of white quartz (48), 6 or 8 inches thick.

Felspar, Quartz and Chlorite Mica.

14, Found in the space contained by Le Hoc St. Clement's, Granville church, and thence eastward.

Felspar, Quartz and Common Mica.

53, From half-way between Plemont and Greve-de-Leiq to X Bay. At Greve-de-Leiq there are two large veins (54) of mica 10 feet wide; close to, and parallel to one of them, is another vein of argillaceous schist.

With the exception of the green porphyry, all the preceding felspar rocks have a peculiarly columnar structure, especially Nos. 33, 36, 41, 42, 43; the finest example of this lays between Le Corbière and the western point of St. Brelade's Bay. They form hills of from 400 to 200 feet in height: the green porphyry no where exceeds 200.

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The stratification, in general, is very perfect, though by no means so much so as that of the argillaceous schist. Fibrous malachite is found in small quantities in the green porphyry; the constant and plentiful occurrence of the black oxide of manganese, in the same, has been already noticed. There is a vein of clay-iron ore in X bay, and the oxide has been found in small detached pieces in different parts of the island. Sulphuret of iron is extensively disseminated in minute grains through the sienite and porphyries; hence, though water is abundant even in summer, it is not unfrequently contaminated by this mineral.

Few rocks surpass those of felspar as building materials, of the utmost durability; they are, however, frequently too hard to repay the expense of being worked as squared stones. At Fort Regent, all the ashler was obtained from near the Icho Rock; the copings, &c., *a*, from Mount Madou, whilst the rock on the spot only afforded rough materials for coarse work. St. Brelade's church (built 1111) is in existence to this day.

Breccia.

Extending from *p* in Boulay bay to *d* (a house half a mile from St. Martrés, bearing 7° west of the church), thence to *e*, on the Rozel road, about a quarter of a mile from the church: *f* is on the St. Catherine's road, about one-third of a mile from the Martello Tower, and *r*, the extreme point on this side, is nearly in the middle of the bay. It also appears at *d*, nearly in the centre of the island, interposed between the felspar and argillaceous rocks. It is a well stratified rock.

From *p* to Rozel Harbour the planes dip north, from the last mentioned point to La Coupe and Verclut, south; so that the turn of the saddle is somewhere in Rozel Harbour. The general height of these hills varies from 300 to 200 feet.

It is *overlaid* by the felspar rocks in all directions; and although formed of all the ingredients already mentioned, without any apparent order of seniority, they are of course to be referred to an earlier date.

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I have observed five varieties.

Argillaceous pebbles		in an argillaceous matrix.
Sienitic	do.	
Porphyritic	do.	
Argillaceous	do.	in a sienitic matrix.
Sienitic	do.	

6, 9 and 9 *a*, are illustrative of most of these when freshly cut: the argillaceous varieties of this rock are hard enough; but they all fail exceedingly on exposure to the weather, the matrix more perishable than the clay-pebbles it contains, which, however, betray their origin, by shaling and splitting after a short time. These last, as well as the sienite-pebbles, sometimes weigh several hundred weight, and are as thoroughly rounded as the small ones.

Sulphuret of iron and manganese still maintain their ground, the latter enveloping the pebbles with a thin film, and with the former penetrating the matrices (6), although I heard the best character, for quantity and quality, of the water of this rock.

Diluvium, or Sea Beach?

The rocks on the shore, from Rozel Harbour to Gouray at Le Hoc, under Fort Regent, from thence to the second Martello Tower in St. Aubin's Bay, from Blanc Pignon to La Carriere, and at L'Estac, emerge from the diluvial deposits lying at the bottoms of the vallies and round the bases of the sea-side hills, forming abrupt banks from 20 to 50 feet high, and invariably composed of the debris of the nearest rocks respectively.

The lower strata of these banks often consist of well-rounded pebbles: this fact is well-exemplified at the little bay *a*, at L'Etac, where the superincumbent debris is at least 50 feet high, and is a thorough mixture of the two rocks, which come into contact at this spot.

The hills are capped in general with a sandy clay (55) intermixed with debris of the contiguous hills.

The flat ground at the mouth of St. Peter's Valley is a complete morass, so that I could obtain no information respecting it; but from what I have observed at St. Aubin's Pier, as com-

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pared with that under Fort Regent, I have no reason to suppose that it varies in its nature from that of the soil in the St. Helier's Valley, and in the contiguous flats extending to near Mount Orgueil. Respecting these last, I have been able to procure the following sections:—

Well at Elizabeth Castle.

18.6 Sandy, fine clay, (see No. 54).
6.0 Debris, angular.
4.0 Pebbles } containing the water.
4.0 Blue clay }

Well at Fort Regent,

236 feet deep; all sienite; water soon found; but continued till 30 or 40 tons per day came in from all crevices and corners, not from any particular stratum.

West end of St. Helier's—sandst.

7.0 Sand.
10. to 15. A mixture of vegetable earth and stones.
8.0 Sandy clay, No. 54.
4.0 Blue clay.
8 to 12 Gravelly clay, in the lower .5 of this the gravel was clean, water-worn, and containing an indifferent water.

Centre of St. Helier's, Mt. Le Capelain.

8.0 Sand.
16.0 Blue clay intermixed with strata of No. 54; in this, at 15 feet below the surface, hazle nut shells and a boar's tusk were found.
2.0 Turf.
2.0 Rounded gravel, mostly sienite. Water impregnated with sulphur. Rock.

Upper end of the Valley. (near *g*.)

2 feet vegetable earth.
12.0 Sandy clay, No. 54.
16.0 Angular debris, clean, and containing abundance of water, but not of the purest quality.

Pier, on blue clay.

All the valley waters throw down a precipitate, adhering to the vessels in which boiled; and some of them are occasionally covered with a film of sulphur. By all accounts, the nature of the ground in the flats, that reach from Fort Regent to Grouville, bears a close resemblance to that which has been just described; except that water is more uncertain, and when found, generally brackish; sometimes 80 feet have been bored in vain, through sandy, blue, and whitish clays; and at other times water has been met with at 16 feet below the surface, as near St. Clement's Church.

Sand.

The flat grounds on the southern and western coasts are liable to the encroachments of the sand, blown over them by

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the winds: this is particularly the case in St. Owen's Bay, where a considerable tract (called Les Quenvais) has been thus ruined. Tradition (for no authentic records can be procured, as those of the Jersey States hardly reach to an earlier period than 1500) states, that an inroad of the sea overwhelmed a large extent of ground in this quarter; and that the sand, no longer kept in check by its old barrier (a wood of oak trees), swept over the space marked *q*, *q*, *q*, until checked by the St. Aubin brook; stumps of oak trees, generally in good preservation, are found lying in the sands near L'Etac. This circumstance of a stream of water, however inconsiderable, arresting the progress of the sands, is further illustrated at Greve de Lecq, and at St. Perranzabuloe, in Cornwall. Might it be turned to account at Bayonne, where the encroachment of the Dunes is so very formidable? The Quenvais originally extended as far St. Peter's Church, still controlled by the brook. Much, however, of this ground has been reclaimed by the persevering industry of the Jersey farmer. In one instance pointed out, a tract of sand-hills, only levelled five years since, now produces parsnips and mangel-wurzel of the largest size, and lucerne, in a soil of no greater depth than that of six inches, formed of vraic (sea weed) and such vegetable mould as could be collected. The black mould, overwhelmed by the sand, and forming the original surface of the ground, varies from two to three feet in depth. Some of it has been cleared and dressed with vraic, or lime, at the rate of four hogsheads per acre, but the expense has been found too great.

The soil on the island is generally very fertile; the best lies in the central parts included by a belt reaching about one mile inland from the sea-coast. Of this central tract, that on the argillaceous schist, by all that I could collect, is of a lighter cast, and, on the whole, inferior to that on the felspar rocks. The soil at Trinity ranks first; it is a stiff loam, which, for agricultural purposes, is ploughed up with sea sand and the gravel from the decomposed sienite. The tract on the green porphyry round St. Saviour's, that about La Hogue Bie and St. Clement's, are in the next best estimation.

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CHAUZEY.

A GROUP of small islets and rocks, eight miles N. W. of Granville, about eight miles long and two and a half wide at an average. Whatever irregularity may be presented by the Jersey rocks as to the number and proportions of the granitic ingredients, those of Chauzey, though similar on the whole, differ distinctly from the above in the uniformity and more equal proportions of their constituents, in their unmanageable irregularity of stratification, in the rounded forms, and frequently gigantic size of the more or less detached blocks on the surface, and in the singular tendency of these last to scale off, when attacked by the elements, in coats concentric to their external and rounded surface.

This rock is almost invariably such a mixture of quartz, felspar, and mica, as is exemplified by Nos. 59 or 53. Sometimes, but comparatively seldom, the felspar and quartz assume a dark red colour, as No. 60. Another, although a minor point of difference, is, that this rock at Chauzey contains very numerous isolated portions of semi-transparent quartz, not exceeding perhaps 20 cubic inches, and also of closely aggregated mica, occasionally containing schorl (61): 59, *a*, is a specimen resembling grauwacké, from a block imbedded in 59. As to the stratification, it is very *marked*, often as much so as the newest rocks, but varying in every direction.

With reference to external form and size, the blocks have exactly the round boulderstone appearance, and often the arrangement of the Cheesewrings; sometimes the detached are nearly cubical, having a length of side of 12 feet.

In fact, the whole group is one quarry of the finest building material of any dimension: Granville and St. Maloe's are built

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of this stone. The Minquiers and remaining islets (on good authority) are composed of the same rock.

The want of leisure prevented my examining the rocks of Chauzey with the same minuteness as those of Jersey; and on the French coast, being still more limited, I was obliged to relinquish the second geological object, actual boundaries and extent, and confine myself almost exclusively to the primary one, order, as I found them between Coutances and Mount St. Michael.

Coutances itself stands on a slate hill, strata dipping south; proceeding south, and crossing the Soulle, you still find slate, but overlaid at the foot of the hill by quartz, pebbles, and a conglomerate (63) of red sandstone, in a matrix of the latter dipping west, and capped by a diluvial deposit of the same. One mile and a half on the direct road to Avranches, brings you to a species of siliceous oolite, of which the specimen 64 shews the largest grained sort that I could find; there was a finer kind very closely resembling, in texture, the regular calcareous oolite: in fact, a variety of sandstone. The broad top of the first hill, south of Coutances, is a heath, under the surface of which, to the depth of a few feet, debris of 64, imbedded in 67, lie on the parent rock, which, on the Granville road, and on the same hill, becomes 65, intermixed with 62, and alternating with it 67, and 68, dipping north. Still following the Granville road, at the foot of the above-mentioned hill, this red sandstone proves itself to be the old red sandstone by overlying limestone, 69, which, from its madreporites, (these I found in a large polished slab from Mount Martin) corresponds to the transition limestone at Plymouth. According to the geological map, in Coneybeare and Phillips, this rock, in Devonshire and North Wales, is found in long stripes, and it maintains the same character here; lying east and west, extending (according to the best information I could collect) a few leagues only inwards, and not exceeding two miles in breadth: 66, is obtained two leagues west of Coutances; I had not time to visit the spot.

At Hienville, this limestone is underlaid by another red sandstone, dipping east. Where this overlies the slate I cannot say, as the hill recedes from the road, which, further on,

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by having ascended the summit of the coast-range, affords no means of ascertaining the precise junction. This slate, as well as the limestone, dips north and west; it corresponds, in every respect, to the Plymouth transition slate in external structure, and internal composition: at Granville it becomes decided grauwacké, sometimes as 73, and the conglomerate * 72, when immediately at the peninsular on which the town stands, and at other times 70 and 71. At 73, it is intersected in every possible way by minute and very abundant quartz veins. These last, and the numerous pebbles of the same, occurring in the conglomerate, together with the very siliceous character of 73, prepares the way for what, perhaps, was the origin whence the veins were deposited, and the pebbles torn, *i. e.* quartz rock †, 75, 76. This is found one mile south-east of Granville, on the south side of the valley, under the road to Avranches, in cliffs of such dimensions as to warrant the possibility of its having some extent, especially when the large deposits of the same, *en debris*, covering the flat hill-tops near Granville, are taken into consideration. It dips north and west, underlying the grauwacké slate, which completely intercepts it seaward, sometimes in the shape of 74, at other times as usual.

About half way between Granville and Avranches, the slate ceases, as it overlays the rock 77, 79. Avranches itself stands on a high abrupt hill, composed of this last and 78 (of which I am quite uncertain as to the extent). Following the road to Mount St. Michel, at the ferry, you again find slate (80), dipping south-east ‡, lying east and west, covered with a debris of mica slate, &c. 79 *a*. The mount itself is composed of 79 and 81, and as this last contains quartz, felspar, hornblende, and mica, there can be no objection to terming it granite. On the authority of my guide, from its external appearance, and correspondence of situation, I have good reason to think

* I cannot leave this without pointing out how completely this conglomerate, in the siliceous extreme of the clay state, seems analogous to the Jersey breccia, as accompanying the argillaceous variety.

† This rock is, probably, at no great distance from the west of Jersey, (compare 22 *a*, and 22 *b*, with 70, 71, 73) as the beach at St. Ouen's Bay is, in a great measure, covered with its pebbles.

‡ Apparently overlying the Avranches rock.

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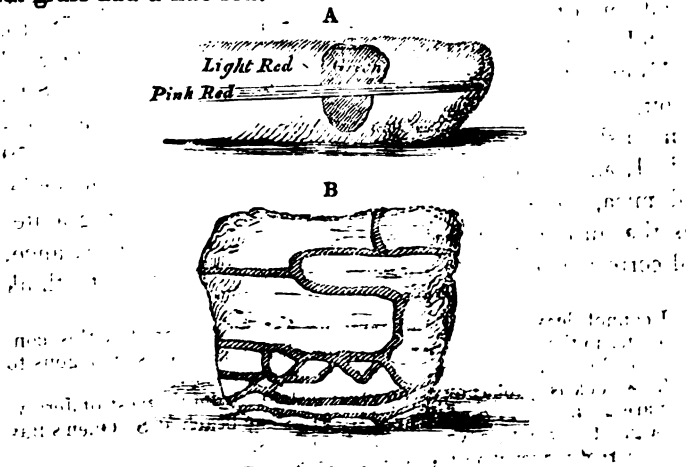
Tourbelaine consists of the same rock as Mount St. Michel: I regret that the state of the tide did not allow me a personal inspection.

The coast-line to the north of, and in the vicinity of Granville, presents an interesting study of one mode of the natural and gradual conversion of vertical cliff into low and sloping hills. From A to B, the cliffs are generally precipitous enough, partly from having been scarped under the fortification, and partly naturally: at B the sand begins to form, not only an ordinary beach, but also an accumulation, as a glacis, 6, which thus immediately depriving the sea of access to the foot of the cliffs, (from which it not only tears out fragments, but carries them off,) and also forms a hollow, 7, which, sloping northward, forms an irregular, but on the whole, connected drain, *e*, of ponds, which (as in the Jersey Quenvais) entirely prevents encroachments of the same sand, that has already proved an efficient barrier to those of the sea.

Returning to B, and following the course of the hills, the now undisturbed ecroulements from the top of the cliffs, successively and gradually present themselves as the ramps $\begin{matrix}1 & 2\\ 1' & 2'\end{matrix}\Big\{\begin{matrix}3 & 4\\ 3' & 4'\end{matrix}\Big\}$ &c. until at about two miles from Granville the rock entirely disappears, under such a slope as 5° 5′ 5″ covered with grass and a fine soil.



B, red veins in green.

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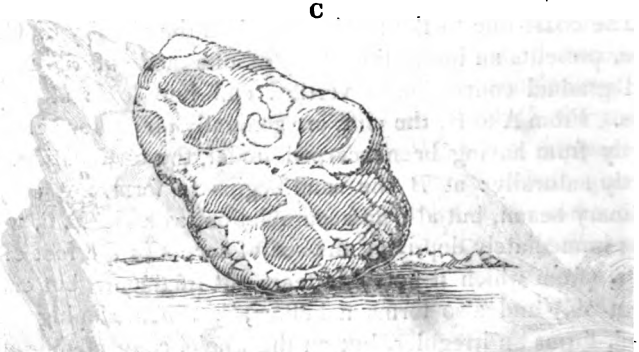
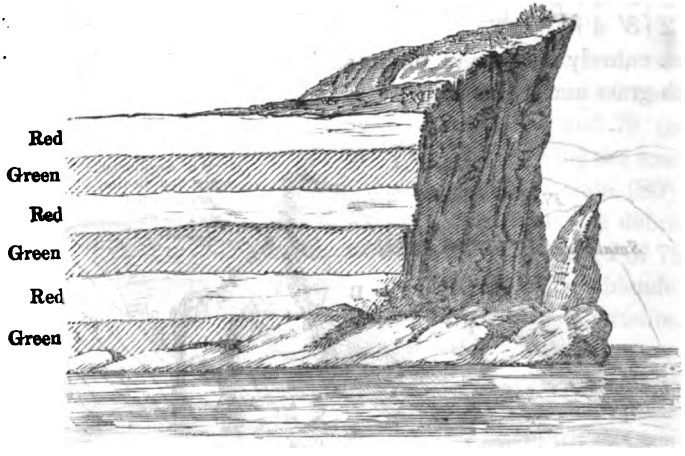
C



The shaded parts always slightly rounded, as if somewhat water worn; neither do they by any means bear the same aspect, although sufficiently so to conclude that they had a common origin.

D



Care must be taken, in examining the above Figure (D), not to mistake the lines by which the Green is distinguished from the Red for cleavage lines.

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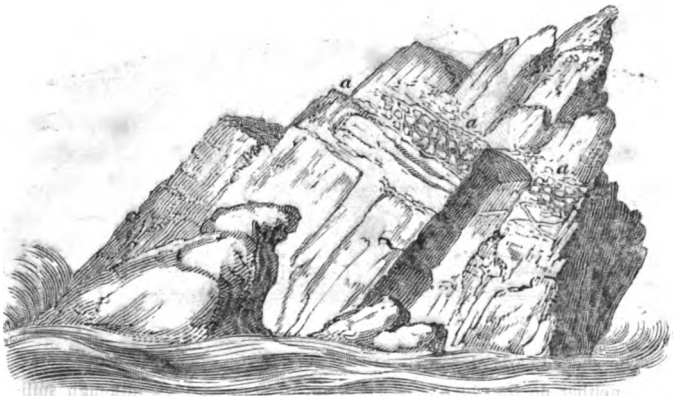
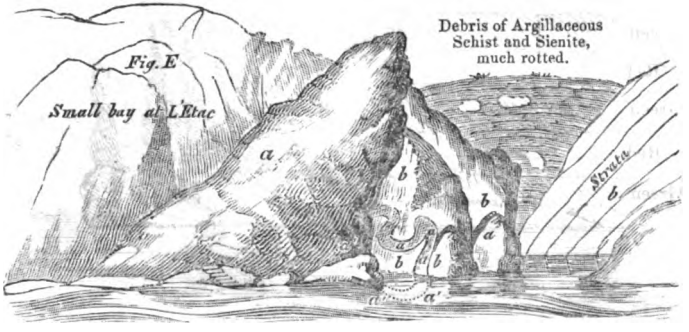


At *a*, the rock assumes the appearance of *fig.* B; the remainder well exemplified by C.

a, has precisely the look of having been shattered by percussion, but the fragments scarcely dislocated.



d, Sienite entering the Argillaceous Schist (*b*) by the vein *a' a' a'*.

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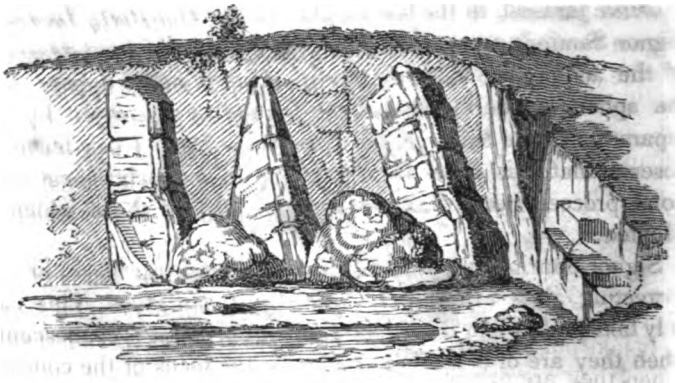
F



Columnar appearance assumed by No. 36, in a quarry in the Trinity Valley.

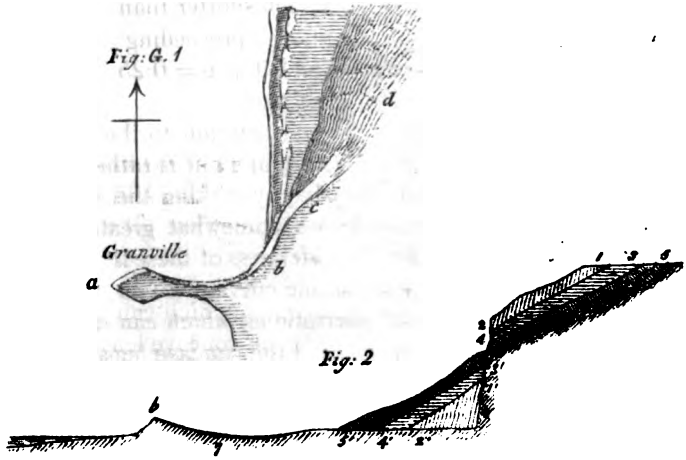


Fig. 2, is a general and considerably enlarged section of Fig. 1.

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On Achromatic Telescopes.

HAVING perused, in the last number of the *Quarterly Journal*, Signor Santini's very scientific discussion on a new construction of the achromatic telescope, from which it would appear that the spherical aberration cannot always be destroyed by the separation of the lenses of the correcting glass, I beg leave to observe that this arises from the data upon which his calculations proceed being somewhat different from those which I adopted.

Signor Santini supposes that the foci of the two lenses of the correcting glass are exactly equal for mean rays. This can only take place when the thickness of these lenses is evanescent: when they are of a sensible thickness, the focus of the concave must be shorter than that of the convex, otherwise their foci cannot coincide, and the combination cannot act as a plane, but as a convex lens; and it is expressly stated in the paper which was read before the Astronomical Society, that to make allowance for the thickness of the lenses, I assumed the focus of the concave lens a quarter of an inch shorter than that of the convex. Our calculations, therefore, proceeding on the different suppositions of $9 + v = 0$, and $9 + v = 0{\cdot}25$ inch, have led to different results.

It is not necessary to limit the construction to the condition of the correcting lens acting as a plane; it is rather advantageous to render it slightly concave by making the difference of the foci of its component lenses somewhat greater than is necessary to compensate for the thickness of these lenses. This enables us, with the smallest possible curvatures, to create an excess of concave spherical aberrations, which can *always* be removed by reducing the aperture of the concave lens, and this is done by separating the two lenses.

I consider myself indebted to Signor Santini for the interest he has taken in the construction; and should be happy if so eminent a mathematician were to repeat his calculations, assigning a shorter focus to the concave lens; when, I hope, our results would no longer disagree.

If the destruction of the aberrations could not be completed

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by the methods proposed, the construction of the correcting lens would certainly demand that great care which Signor Santini considers necessary; for it would require, as in the ordinary construction of an achromatic, a very nice adaptation of the foci and curvatures of the lenses to the refractive and dispersive powers of the glass. But the changes of position, by changing the apertures, and, consequently, the aberrations of the lenses of the correcting glass, give us so great a command over these aberrations, that this construction neither requires a very accurate knowledge of the refraction and dispersion of the glass employed, nor a great nicety in the foci and curvatures of the lenses; and, therefore, it is much easier in practice than the common construction.

This I have verified by actual experiment; having had a telescope made upon this principle, calculating from the average values of refraction, assuming a dispersive ratio somewhat lower than is met with, and making the focus of the concave lens superabundantly short. Having, by these means, given to the correcting lens a surplus of concave spherical aberration, and a surplus of dispersive power, I was enabled, by varying the positions of the lenses, to reduce these surplus aberrations, so as to render the performance of the telescope not inferior to that of an achromatic of the ordinary construction. The object lens is of crown glass, 61½ inches focus, and 5 inches aperture; the convex of the correcting lens is also of crown, the radius of each surface 9 inches, and the aperture 3 inches; the concave is of flint glass, the radius of the first surface 9 inches, that of the second surface 10.4 inches; the distance of the correcting lens from the object-glass is 25.1 inches; and the separation of the lenses 0.16 inch. The correcting lens acts as a concave, extending the focus to 64.7 inches. The two convex lenses were originally of plate glass; but they were so veiny and imperfect in other respects, that I had them replaced by two others of crown glass, made by Mr. Dollond, in whose hands the telescope was a short time ago.

The danger of deranging the centering of the lenses, by the minute separation required, does not appear insurmountable by very simple mechanism; two tubes, sliding steadily upon one another by the action of a screw, seem sufficient for the

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purpose. One advantage, however, might be obtained by giving up this facility of adjustment; for by making the interior surfaces of the correcting lens to coincide, and by cementing them together, we could prevent all loss of light by reflection from these surfaces. Nor does the construction appear, even with this limitation, so difficult as the ordinary one; since the alteration of one surface only, namely, the second of the concave lens, would be necessary. If the curvature of this surface were too great, the surplus of concave aberration would have to be removed by flattening it, instead of separating the lenses.

ALEXANDER ROGERS.

Leith, 28th October, 1829.

Chemical Examination of a Native Arseniuret of Manganese. By Robert John Kane, Member of the Meath Hospital Medical Society, Dublin.

SOMETIME since I obtained from a person, who had a collection of minerals for sale, a number of specimens of various ores of manganese, amongst which was one ticketed "Manganese Ore, Saxony." It was sold me as a specimen of the native peroxide. It was rather a small piece, about two and a half ounces weight, and being based on a mass of foliated galena, was pierced, in every direction, by small veins of ferruginous quartz. I did not take much notice of it at the time, but some months afterwards, being desirous of comparatively examining the different specimens of the native oxide which I possessed, I exposed, in a tube retort, to a red heat, a few grains of this mineral. No oxygen came over, but I was much surprised to see that a substance rose in vapour, and condensed under the form of brilliant acicular crystals on the inside of the cool portion of the tube; I immediately removed the retort from the fire, and in so doing, it cracked. The external air got admission, and the mineral inflamed, burning with a blue flame, throwing off copious white vapours, and emitting a strongly alliaceous odour.

I was surprised at this phenomenon, as there had been no

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native arseniuret of manganese described by mineralogists, therefore I resolved to more thoroughly investigate the subject; and after the most careful study of the nature of this substance which I was competent to effect, I can reconcile the appearances which I observed, to no other supposition than that which I before mentioned.

External Characters.

The specific gravity of a homogeneous fragment was found to be 5.55.

Hard, brittle, perpendicular fracture, uneven, fine-grained, brilliant; colour greyish white, growing dull, and becoming covered with a fine blackish powder on exposure to the air; horizontal fracture, dull and mammillary. It is very easily broken in this direction; in fact, the ore seems entirely composed of a series of mammillary laminæ.

Before the blow-pipe, it burns with a bluish flame, and emits a smell similar to that of arsenic, when heated strongly. The greater part of the ore sublimes. Whilst the ore is burning, it throws off white fumes which condense on the cold part of the charcoal, under the form of a white powder. When a platina stand was used, the mineral fused and united with it.

This ore is totally dissolved by nitro-muriatic acid.

When boiled in a large quantity of nitric acid it is entirely dissolved; but when the quantity of acid used is small, the ore is converted into a white powder, soluble in more acid.

When re-agents were applied to a solution of this mineral in nitro-muriatic acid, the following effects were observed:—

Alkalies precipitated it white, which gradually passed to brown.

Carbonated alkalies produced a precipitate; the colour of which was more permanent.

Lime-water and

Solution of acetate of lime produced white precipitates, in a portion of the solution which had been rendered neutral. An excess of acid re-dissolved these precipitates.

Tincture of galls did not affect it.

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A solution of ferro-cyanite of potassa tinged the liquor of a light-blue, but did not produce any well-formed precipitate.

Hydrosulphuret of ammonia precipitated it of a dirty yellowish white.

A portion of the ore having been heated in a glass tube, the sublimed metallic crystals were separated for examination.

They dissolved perfectly in nitric acid, and the solution being concentrated to expel the excess of nitric acid, was found to possess the following properties :—

When a current of sulphuretted hydrogen was passed through it, there was produced a yellow precipitate.

When neutralized by potassa, and nitrate of silver added, the well-known brick-red precipitate of arseniate of silver was produced.

With nitrate of copper the neutral solution gave a greenish precipitate ; and with hydrosulphuret of ammonia, one of the yellow colour of orpiment, when rendered slightly acid.

After several preliminary trials I resolved upon employing the following process for analytically determining the relative proportions of the constituents.

The ore was digested in nitric acid until it was entirely converted into arseniate of manganese ; to it was then added an excess of a solution of potash, and the liquors were boiled for some time longer. The arseniate of manganese was thus decomposed, and the protoxide eliminated, rapidly absorbing oxygen, was converted into the brown deutoxide of that metal, which being dried and weighed, the quantity of manganese was calculated from it.

The liquor thus freed from the manganese was accurately neutralized by nitric acid ; then a solution of binacetate of lead was added, as long as any white precipitate was produced. The arseniate of lead, thus formed, was dried, weighed, and the quantity of arsenic deduced from it.

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From the result of three different analyses, I was induced to adopt the following numbers as the average:—

Manganese	45·5
Arsenic	51·8
Loss and a trace of iron .	2·7
	100·0

But this is somewhat an approximation to the numbers of manganese and of arsenic combined atom to atom; and perhaps the difference depends rather on my unskilfulness than on the real composition of the mineral. The relative equivalents might be so arranged, viz.—

	Atomic Weight.	Theory.	Experiment.
Manganese	28	42·4	45·5
Arsenic	38	57·6	51·8
			Loss 2·7
$Mn^{g} + A^{rs}$	66	100·0	100·0

A Plan for Improving the Carriage Pavement of the Metropolis. By Lieutenant J. H. Brown, Royal Navy.

Having observed the unsafe and disgraceful state of the pavement of most of our public streets, and, even where Macadamizing has been adopted, the repairs continually required, and the constant accumulation of mud and dust, I have given the subject much attention for the last few years, and I am induced to propose a plan to remedy the defect.

I must premise by observing, that the foundations of most of our streets are, of necessity, bad; as, independent of the nature of the soil, the ground has been so often dug up and turned over, for the purpose of levelling, forming and repairing sewers, laying down gas, water-pipes, &c. &c.; that although the paving should be executed in the best manner, it is impossible that it can remain in the position placed; for the water, after a glut of rain, passes down between the joints of the stones, the foundation, composed of earth, rubbish, &c. speedily becomes a puddle, *softer* in some *parts* than in *others*, a great portion of which works and churns up between the joints, and allows the stones to sink under the first heavy carriage that

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passes, forming pools for the reception of more rain, and daily becoming worse, until it is necessary to relay it; when, after a short time, the same thing happens over again, thus causing an endless expense to the different parishes.

The plan I suggest is, that, after the foundation has been formed in the necessary shape, and the surface rolled or rammed hard, the paving stones, dressed so as to fit close together, should be laid or set in a *thick coat of good mortar*, and the joints grouted with cement; the *whole mass* would thus become a *solid body*, and the rain would be effectually prevented from penetrating to the foundation, which would remain dry and firm in the position it was originally placed.

I recommend this in preference to any other artificial foundation, such as broken stones, sand, gravel, &c.; for, provided it was sufficiently firm to bear the weight of the paving stones and carriages passing, nothing would displace it until worn out.

The stones must be grooved on the surface about a quarter of an inch deep, to prevent horses slipping; and if scored diagonally, it would obviate any jar or jolt that could be anticipated from the wheels of a carriage passing over them at right angles. This scoring could be renewed when necessary, by providing light screens on wheels, inside of which the men could work on any part with hammer and chisel, protected from danger. Carriages would run over this pavement with far less friction, and as little noise, as they now do over a Macadamized road; and this great advantage would be gained, that, whereas the one is constantly wearing in holes and wanting repairs, never looks finished, and covers the inhabitants and travellers with mud and dust, (the only change) a pavement laid down on this principle would last for many years without further expense or trouble; and as no mud could work up from the foundation, consequently no dirt could accumulate, but what fell from the horses; and the small, but gradual and even wearing away of the surface, it could be kept by sweeping as clean as a barn-floor; the expense of watering in summer would also be saved.

This plan would be attended with but little more cost than

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the old mode of paving, the difference being the dressing the stones, and substituting *mortar* for *gravel;* certainly, considerably cheaper than the method adopted in many parts, of laying a foundation of Macadamized stones. And to prevent the necessity of ever disturbing it for any purpose after it was once made, I would construct culverts * along the sides of the streets, under the part now occupied by the surface-gutters, large enough to contain the branch water-pipes, gas-pipes, or, indeed, anything that could be conveyed through pipes in or out of the houses. The tops of those culverts should be of cast iron, about four feet long, and each length, if necessary, (or wherever a joint or communication was required) should *lift on or off* at pleasure. The inspector or workmen could thus, at all times, have easy access to the pipes, without interfering with the street or house; and any accident could be amended cheaply and promptly, (the men having day-light and room to work) without the expense and annoyance of breaking up the pavement, which could never be properly replaced. The upper surface of those cast-iron lengths should be hollowed into the form of a gutter, in order to carry the rain water from the street to the grating which communicates with the sewer; and the bottom of the culvert would convey any water that leaked through the joints, in the same manner.

For a new pavement, I would recommend the best material (granite) to be used, as the cheapest in the end; but in relaying a street, the same stones may be used, after being dressed or cut to the necessary form. I consider the best proportions to be eight inches long, five inches wide; and a foot, or as much more as convenient, deep. If it was necessary to use different sizes, they must on no account be mixed, but worked into separate lengths or patches of pavement.

* The mains as they are at present constructed, may run through the centre of the street; as they would seldom or ever want to be disturbed, and they could be inspected when the pavement was worn out, and in the progress of relaying; or they could run along the sides, if the culverts were made large enough; of course the size of the side culverts or gutters, depends on the situation and the quantity of water required to be conveyed.

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Although Macadamizing is undoubtedly a great improvement on our roads, it can never come in competition with this mode of paving for streets, or great thoroughfares, which has all the advantages of *strength, cheapness, beauty, durability, and cleanliness.*—June 25th, 1829.

Since making the above observations, I have noticed that this method has been *partly* adopted in different streets in towns; that is, so far as dressing the stones and grouting the joints, a considerable step towards improvement; *but this of itself is not sufficient*, for if a stone rests on earth, gravel, or broken stone, for the bed, it is liable to be disturbed by the shock of a heavy carriage passing, in which case the grouting in the joint will crack, and admit the wet to the foundation, causing all the mischief I have already described. Another very bad practice I have observed, is that of ramming the stones, to bring the pavement to a level, some days after it has been grouted, and in reality destroying the benefits to be derived from it, as good mortar or grouting should set and get hard, in a very short time after it is first applied, and the ramming cracks the joints again: besides, all the foundations I have seen are *porous*, and although being for the most part of broken stones, they will not work or churn up, as the gravel does, (which accounts for the mud we wonder to see in the streets that are so often swept), still, they allow the *water* to *pass* through them to the *real* foundation below, causing the pavement to sink into inequalities, as may be now seen in Fleet-street, which was done at a great expense about two years ago.

By *bedding* the stone in mortar, properly *placed in the situation it is to remain*, then grouting the joint, and allowing it to set hard, without *afterwards ramming or disturbing it*, the pavement will remain *immoveable* and *water tight*, until fairly worn out, and save all the expense of an artificial foundation of Macadamized stones, or other matter. Now, when we consider the labour of digging out, and carting away, eighteen inches or two feet deep of earth, and replacing it with

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another material to be provided, prepared, and carted also; such expense, which I maintain is all *unnecessary*, and which must make the *principal item* in the cost of payments I have lately seen, may all be saved by this plan.

A grand objection to a Macadamized pavement, in this and every cold climate, is, that a severe frost setting in after wet, does *incalculable* injury, owing to its porous state; now, as no water can penetrate *beneath the surface* of this pavement, if properly made, this serious fault is obviated.

I trust the strength and cheapness of this plan, as compared with all others I have seen, will induce some one, having the power, to grant it a trial, and I have no fear for the result. I have observed that the most useful inventions are the most simple and obvious when once made known; and often, from their very simplicity, take away from the merit of the inventor; we may instance steam, gas, &c. &c. As I publish my sentiments and observations, and the result I have come to, after much trouble and attention, without any sinister motive, I trust I may escape the imputation of having promulgated a plan which any one could suggest; should such casuists be found, I refer them to the story of Columbus breaking the egg.

September, 1829.

On some Pharmaceutical Preparations of Iron, and particularly the Tartrates. By Andrew Ure, M.D., F.R.S., &c. &c.

1. Tartaric acid has hardly any action on the red oxide of iron, for though 200 grains of the former dissolved in water, were digested on a sand bath on 50 grains of the latter, in the form of the rubigo ferri of the shops, it became but faintly coloured in the course of three days, and a very few grains only of the oxide were taken up. The same rust of iron was quite soluble in dilute muriatic acid.

When tartaric acid, in solution, is digested on red oxide of iron, prepared by nitric acid, no apparent combination ensues

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after many hours, and the re-crystallized acid is nearly colourless.

2. The readiest mode of obtaining a proper red tartrate of iron, is, by mixing the liquid red sulphate with solution of tartrate of potash in equivalent proportions. Sulphate of potash precipitates in a crystalline powder, (the solutions being somewhat concentrated) which may be separated from the blood-red liquid tartrate of iron by filtration. When to this ferreous solution, its own bulk of alcohol, sp. gr. 0.840 is added, so as to form a proof spirit menstruum, decomposition immediately ensues, indicated by a cloudiness, and a precipitate of a treacly consistence and aspect, which collects at the bottom. The supernatant liquid is nearly colourless, and contains hardly any iron, but much tartaric acid. The viscid precipitate soon hardens into a brittle mass of subtartrate of iron, insoluble in water. Thus it appears, that a spirituous menstruum is not at all adapted for holding red tartrate of iron in solution, though Madeira and Teneriffe wines of common strength answer very well.

When the above concrete precipitate is treated with water, acidulated with tartaric acid, it readily dissolves, with the reproduction of red tartrate of iron.

The subtartrate, when newly thrown down, is fusible, at the heat of 180° or 190° F. It burns reluctantly in the flame of a spirit-lamp, with a faint ignition, and a slight smell of caromel.

3. The potash-tartrate of iron, as prepared by the process of the London Pharmacopœia, is a powder of an olive-green colour, occasionally tinged with brown. When 100 grains of it were heated to the temperature of 160° F., they lost 4 grains; but this loss will vary according to the manner of preparing it. By dull ignition, in a platinum crucible, it emits a lambent blue flame, and is converted into red oxide of iron and carbonate of potash, amounting together to 52 grains. The alkali was dissolved out with water, and tested with acid. It indicated 18 grains of potash, equivalent to 46.5 of tartrate of potash. The

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peroxide of iron weighed 32 grains. This existed in the original compound, partly as a tartrate, and partly as a subtartrate; for not more than two-thirds of the original powder are soluble in water.

If one volume of the solution of the potash-tartrate (in about seven times its weight of water) be mixed with one volume of alcohol, sp. gr. 0.840, so as to form a proof-spirit menstruum, the subtartrate of red oxide of iron immediately forms, and falls in a viscid mass, and the spirituous liquid becomes nearly colourless, containing very little iron.

Though the spirituous vehicle, prescribed in the Pharmacopœia, be weaker than proof, there can be no doubt from the above experiments, that a dilute alcohol is not nearly so proper a menstruum for this triple salt of iron as Madeira wine, which, containing a considerable portion of acid, will form a more powerful and permanent solution.

4. Medical men have, in modern times, probably paid too little attention to the state of oxidizement in which they administer iron. The older chemical physicians of the celebrated school of Stahl, taught, and I believe justly, that, according as this metal is differently prepared, it acquires powers over the body of a different, and almost opposite, nature. Some preparations were said to promote the motion of the fluids through the whole system; while others repressed or obstructed these motions. The remarkable stimulant and deobstruent virtue displayed by iron in the cure of chlorosis, was, at that period, attributed to one of its supposed constituents, the *phlogiston;* as the astringent property was referred to the *earthy* ingredient. When these notions, derived from "old experience," are expressed in modern phraseology, we may say, that the mildly exciting power of iron will be found in its metallic or protoxide state; while its acrid and constringing qualities may be sought for in its peroxide, and in certain saline compounds, where the acid contributes its share of the effect, as is the case with the sulphate.

In fact, it may be affirmed, that iron, like copper and mercury, acquires acrimony (pathologically speaking) by per-

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oxidizement; a conclusion which would have been more generally drawn, had a good form of protoxide preparation existed in our Pharmacopœias, or our shops. The *precipitated subcarbonate of iron* is merely the peroxide associated with only from 3 to 5 per cent. of carbonic acid; and is, therefore, not entitled to its pharmaceutical name.

A very pure, mild, and permanent form of a protoxide-salt may, however, be easily obtained by exposing clean particles of iron, as bits of iron wire, to the action of tartaric acid and water at a gentle heat. An effervescence ensues, hydrogen is disengaged from the water, the iron is oxidized to a *minimum*, and is fixed in that state by its instantaneous combination with the acid of tartar. This tartrate owes its permanence to its insolubility; but yet (like iron filings and calomel) it acts energetically on the system. The prototartrate of iron is nearly white, and pulverulent. The powdery matter, as diffused in the liquid, may be decanted off the iron into a filter, and washed with a little water. It has a mild chalybeate taste, and will constitute a valuable accession to the *Materia Medica*.

At a dull red heat, this tartrate readily takes fire, and burns slowly away like tinder, after its removal from the source of heat, with the exhalation of a caromel odour; while the oxide of iron becomes peroxidized.

Glasgow, December 5, 1829.

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MISCELLANEOUS INTELLIGENCE.

§ I. Mechanical Science.

1. *On Measuring the Force of Pressure, by* Mr. Bevan.—Mr. Bevan describes his highly useful and applicable method in the following manner:—"If we take a leaden bullet of any determinate diameter, and expose it to pressure between plates of harder metal, made to approach each other in parallel position, the bullet will be compressed or flattened on two opposite sides in an equal degree: provided the lead is pure the degree of compression will indicate the amount of pressure. With a graduated press of the lever kind it will be easy to form a scale of pressure corresponding to the different degrees of compression, until the ball is reduced to a flat circular plate of about one-fifth of an inch in thickness; and it will be found, that an ordinary bullet, of about five-eighths of an inch in diameter, will require a pressure of near 4000 pounds to effect this degree of flattening. Suppose, therefore, we wish to measure an actual pressure supposed to be nearly 20 tons, we have only occasion to place 10 or 12 of these balls at a proper distance asunder, so as not to be in contact when expanded, and then to measure, by good callipers, or other suitable means, the compression of each ball either by its thickness or diameter, and afterwards add into one sum the particular pressure due to each ball, from the scale first made, by using the lever press before mentioned."

By this mode I have ascertained the amount of friction of an iron screw press, with rectangular threads, to be from three-fourths to four-fifths of the power applied; or the actual pressure has not exceeded 4 or 5 tons; when the calculated pressure, if there had been no friction, would have been 20 tons.

The larger the ball the greater will be the pressure necessary to reduce it to a given thickness. An ordinary leaden shot of one-eighth of an inch diameter will require nearly 100 pounds to compress it into a flat plate. By using a ball five-eighths of an inch diameter, I have found the actual pressure of the common bench vice to be about 2 tons; when under the same force, if there had been no friction, the pressure would have been 8 tons.

In the practical application of these balls it will be convenient to make a small impression upon them with a hammer before they are placed between the plates, to prevent them from rolling out of their proper position: this operation will not be found to interfere with the result, as it is the ultimate compression only that is sought, and which is not affected by that of a smaller degree before impressed. Hence the same substance may be used several times, provided the succeeding pressure exceeds that of the preceding.

It may be observed, that the application of these leaden balls to

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determine the actual pressure, will not interfere with the regular operation of a press, as the articles under pressure may be in the press at the same time the balls are used, which of course must be placed between separate plates.—*Phil. Mag.* N. S. vi. 284.

2. *Night Telegraph.*—A night telegraph, invented by M. Lecot de Kerveguen, appears from the French journals to attract some attention, because of its novelty, cheapness, and applicability, both by day and night. The inventor has gradually improved it until he can obtain 29,245 signs by its means. It consists of a cabin with two faces, for the purpose of transmitting signals in two directions. These faces are pierced with three circular holes, each divided by a vertical and a horizontal diameter. Each hole is covered by an opaque black disc, in which is opened a ray or line, which is rendered white for day observation, and luminous at night time. Motion is given to the discs, and consequently to the rays from within the cabin, so that right or acute angles to the right or the left, upwards or downwards, may be made at pleasure. The dimensions of the cabin are proportional to the diameter of the discs, and the rays on these are themselves proportional to the distance between the telegraph and its correspondent one.

The following are some of many experiments made with this apparatus: they took place on the 21st of March, at eight o'clock in the evening by bright moonlight:—

The first ray was	4 feet 6 inches long	by 8 inches wide.
second	4 do.	6 do.
third	3 do.	4 do.
fourth	2 do.	3 do.

The telegraph was at the port of Toulon, and all the signals were well observed by the men at Cape Sepet, which is 1½ leagues distant. On the following morning the experiments were repeated with the white rays by day-light. From these experiments it appeared that the smallest ray was abundantly visible at the distance mentioned, and experiments since have shewn at a distance even more than double.

Still more recently, M. Kerveguen has so far simplified his arrangements as to use only one ray or bar of light; and though he has by doing so reduced his arbitrary signs to 8649, yet that is a number abundantly sufficient for all ordinary uses. In the new experiments made the smallest ray was used, and still found fully sufficient for its intended purpose.—*Revue Ency.* xliii. 763.

3. *Chinese Canal.*—A canal was opened in 1825, to the west of Sargan, in Cochin China, which connected that town with a branch of the river Cambodja. Its length was 23 miles, its width 80 feet, and its depth 12 feet. This canal was begun and finished in six weeks, although it had to be carried through large forests and over

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extensive marshes: 20,000 men were at work upon it day and night, and it is said that 7000 died of fatigue. The sides of the canal were soon covered with palm-trees, for the cultivation of which the Chinese pursue a particular method.—*Allg. Handl. Zeitung*, 1825.

4. *Method of removing fixed Glass Stoppers.*—M. Clausen recommends that in these cases a piece of woollen list should be passed once round the neck of the bottle, and the two ends taken by two different persons; then the bottle being held firm, if the persons draw the list alternately towards them, the friction upon the neck of the bottle will soon warm it so much as to enlarge the glass, and allow the ready removal of the stopper. In place of this M. Chevalier recommends the old process of warming the neck either by a hot coal or a flame. He adds, if the top of the stopper is broken off so that no hold of it can be taken, the bottle should, after being warmed, be enveloped in a cloth, so as to leave the neck free, and then be struck upon the bottom (by the hand). Generally the first blow will make the stopper fly out; but sometimes several successive blows are required to effect the separation.

5. *On the Causes of Diffraction*, by M. Haldat.—The phenomena of diffraction, the examination of which, in latter times, has furnished such powerful arguments against the hypotheses of Newton, and drawn philosophers towards the opinion of Descartes, have appeared to M. Haldat as if they had hardly been sufficiently discussed in relation to the circumstances which might modify or elucidate their cause. It is with this in view that he has undertaken numerous experiments, in which the bodies producing diffraction, which he calls diffringent bodies, have been submitted to the action of agents the most proper to modify them, and as an attractive force is the power by which the Newtonians explain diffraction, he used in his trials all those agents most likely to affect it.

After assuring himself, as had been announced by other experimenters, that this phenomenon was not modified either by the specific gravity or the chemical nature of the body, M. Haldat turned his attention to the strongest natural powers; heat, electricity, magnetism, electro-chemical currents, and finally affinity, so powerful in modifying the attractive force, have been successively and simultaneously employed to modify the state of the body, whilst it exerted that influence upon the luminous rays which occasions diffraction, without, however, producing any sensible alteration in the phenomena. Thus metallic wires and diffringent plates of iron, copper, and silver, have been heated to whiteness, and cooled to 14° F.; and yet the coloured bands produced by their action upon the rays of light have not presented an appreciable difference from those produced by the same bodies at common temperatures.

Diffringent wires and plates have been made the channels for

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currents of ordinary electricity for violent discharges of powerful batteries, and for electro-chemical currents sufficiently powerful to ignite and fuse them. Currents moving in the same and in opposite directions have been used. The ray of light has been received on the edges of diffringent plates connected with powerful magnets, yet without any appreciable alteration. The rays of light, before arriving at the diffringent body, have themselves been traversed by powerful flames, by strong electrical currents and discharges, without any change being perceived in the fringes and other appearances of diffraction. The obscure bands in the shade of these wires have remained invariable both in intensity and dimension.

From these experiments, M. Haldat thinks, that the explanation of diffraction, founded on an attractive force, or on certain atmospheres supposed to exist around the body, can hardly obtain the assent of philosophers, when this attractive force and these atmospheres being submitted to agents so fitted to alter them, have produced no change in the phenomena. These facts certainly do not establish the theory of undulations, but they help to destroy the only explanation which can be opposed to it. The author, however, does not hide the difficulties which these experiments also present to the theory of undulations, and inquires how it is possible that the motion of the luminous waves, which ought to be so regular and constant, is not disturbed by the flowing out of subtile fluids which strike against them in their course. He refers the solution of this question to the period when science shall have penetrated more intimately into the nature of those agents which at present are known to us only by their effects.—*Ann. de Chimie*, xli. 424.

6. *On the Impressions produced by Light on the Eye.*—The following are the conclusions to a mémoire on this subject by M. Plateau. *First section*—i. Any sensation of light whatever requires an appreciable time for its complete formation, and also the same time for its complete disappearance. ii. The sensations do not disappear suddenly, but gradually diminish in intensity. iii. As a sensation fades, the progress of its decrease is slower as the effect is nearer to a close. iv. Different colours illuminated by daylight produce sensations differing little from each other in their total duration. Their order, in this respect, beginning with that which produces the longest sensation, is white, yellow, red, blue. v. The total duration from the time when the sensation has acquired its fullest power, to that when it is hardly sensible, is very nearly 0."34. vi. Finally, it results accidentally from the experiments, that the principal colours arranged according to the intensity of the sensations which they are competent to produce, stand in the following order, white, yellow, red, blue.

Second Section—i. New proofs confirm the order of colours contained in the sixth result of the first section. ii. The visual angles

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under which M. Plateau can see the different colours, are as follows:—

	In the shade.	In sun-light.
White	18″	12″
Yellow	19″	13″
Red	31″	23″
Blue	42″	26″

The angles observed in sun-light are nearly a third of those in the shade. iii. When the sensations of two different colours succeed each other on the retina with a velocity less than that necessary to make the two impressions appear as one, there generally appear certain shades which are extraneous to the two colours employed, or to their mixture; by this means a fine white can be obtained when the yellow and blue colours only are used. iv. When two alternating sensations succeed each other with such rapidity that they produce but one impression, the latter does not always present a colour which would result from the mixture of the former; thus, combining the effect of yellow with that of deep blue in the way just mentioned, a grey colour can be produced without the least appearance of green. v. With the exception, perhaps, of yellow, the sensations of certain colours do not act in their combination with other sensations in the order of the intensity of their colours; their maximum of influence exists in a certain pale tint, on each side of which their influence diminishes: thus, the blue colour of maximum power with respect to red and yellow, is that of the sky in its most coloured state.—*Bull. Univ.* A. xii. 123.

7. *Brewster's Monochromatic Lamp.*—Dr. Brewster makes a monochromatic lamp by taking a portable gas lamp and fixing a platina wire, or a plate of mica in the flame; platina wire spirals answer very well until they become covered with carbon. A large cotton wick also, imbued with sal ammoniac, produces a good effect in a hot gas flame. The supply of solution of muriate of ammonia, required to continue the light, may be given by means of a large sponge, or a capillary fountain. Dr. Brewster has used these monochromatic lamps very much in microscopical researches.—*Edin. Jour.*

8. *Artificial Magnets and their uses.*—Extract of a letter from Strasbourg:—Dr. Keil, of Langensalza, has discovered a method of making artificial magnets of enormous force, and infinitely more powerful than those as yet made, without great increase of dimensions. He has also, by their means, cured many diseases dependant upon the nervous system. The most powerful magnets as yet made, have been large, and not supported more than 88 or 90 pounds; but M. Keil has made one which sustains 45 pounds, and yet weighs itself only 3 pounds 6 ounces; he has another which

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supports 480 pounds. This magnet is in the form of a horse-shoe, and is composed of 9 plates; its length is 17 inches, and its weight 43 pounds.

I have myself seen (says the writer) this magnet carry the enormous weight mentioned, and M. Keil says he can make them still stronger, and competent to carry five times this weight, without increasing their size in a greater proportion. He can also strengthen old magnets, but never bring them near to those manufactured upon the new plan.

This discovery is said to be of great importance in the curative art. The magnetic influence upon diseases has been supposed to have been long known, and even cures affected in some cases by making passages over the affected part by means of magnets. But the bars used were too feeble; and it appears that those belonging to M. Keil which produce singular effects, are alone capable of demonstrating the power of mineral magnetism over diseases. He has succeeded in very violent rheumatic pains, epilepsy when not due to organic lesions, cramp in the stomach, feebleness of sight, goitres, head-ache, tooth-ache, tic douloureux, &c. The writer's brother was a witness of one cure effected in a case of rheumatism.

M. Keil also proposes to publish his investigations of terrestrial magnetism, and the theory of magnetism, in which it is said he has much new matter. He is on his way to Paris, so that we may hope very soon to know the truth of the various matters stated, which, if they have any reasonable foundation, cannot fail to be of great importance.—*Bull. Univ.* A. xii. 241.

9. *Application of Magnetism to Medicine.*—Dr. Becker has also published a long account of effects produced by powerful magnets on nervous and other patients. His conclusions are, that, i. Mineral magnetism is a very efficacious agent in nervous pains, especially when they are of long standing; ii. That it is not of any utility, but rather injurious, when inflammation or any other excitation of the vascular system accompanies these pains; iii. That its effect is less sure on recent nervous cases, because they are frequently accompanied by unperceived febrile affections.—*Hufeland's Journal.*

10. *Daily Magnetic Variation.*—According to experiments made by Humboldt, it appears that the daily variation is by no means the same in different places. On January 29th, the variation at Berlin was three times greater than on the 27th; whereas, at Paris on the 29th, it was much less than that of the 27th at Berlin. At Berlin, the variation, on the 11th of January, was twice that of the 10th at Paris; that of the 10th was greater than that of the 11th. These results do not appear to depend on any error of observation, but are real consequences of local causes. By trials made at the mouth and lowest part of the Freyburg mines, it was found that a depth

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of 798 feet in the earth had no sensible influence upon the inclination of the needle.

11. *Use of Muriatic Acid in cleansing Monuments.*—The time and expense necessary in cleansing stone buildings and monuments by scraping them, induced M. Chevalier to try other methods. He finds it may be done, i. by dry brushing, but the process has inconvenience; ii. by washing first with a brush and water, then with water acidified with muriatic acid, and finally by water alone; iii. by washing with a weak alkaline solution, and then with a very dilute acid. The second process is considered as the most advantageous, and has been applied on the walls of the Ecole de Médecine. It is now under trial upon the marble statues of the gardens and halls in the building.

12. *Preservation of Firemen exposed to Flames.*—The Chevalier Aldini of Milan has been earnestly occupied in the construction of an apparatus, or rather clothing, intended to preserve persons from injury who are exposed to flames. The apparatus has lately been fully tried at Geneva, and an account of it, and the trials, given in the Bibliothèque Universelle. A union of the powers possessed by a metallic tissue to intercept flame, with the incombustible and badly conducting properties of amianthus, or other substances, has been made in the apparatus; and the latter consists of two distinct systems of clothing, the one near the body composed of the badly conducting incombustible matter, and the other, or external envelope, of a metallic tissue.

The pieces of clothing for the body, arms, and legs, are made of strong cloth which has been soaked in a solution of alum; those for the head, the hands, and the feet, of cloth of asbestos. That for the head is a large cap, which entirely covers the whole to the neck, and has apertures in it for the eyes, nose, and mouth, these being guarded by a very fine copper-wire gauze. The stockings and cap are single, but the gloves are double, for the purpose of giving power of handling inflamed or incandescent bodies.

M. Aldini has, by perseverance, been able to spin and weave asbestos without previously mixing it with other fibrous substances: the action of steam is essential in the bending and twisting of it, otherwise the fibres break. The cloths prepared with it were not of close texture, but loose: the threads were about one-fiftieth of an inch in diameter, and of considerable strength: cords of any size or strength may be prepared from them. M. Aldini hopes to be able so to prepare other fibrous matters, as to be able to dispense altogether with this rare and costly material.

The metallic defence consists of five principal pieces: a casque, or cap complete, with a mask: this is of such size as to allow of sufficient space between it and the asbestos cap, and is guarded before the face by a visor, so that the protection is doubled in that

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part; a cuirass, with its brassets; a piece of armour for the waist and thighs; a pair of boots of double wire-gauze; and an oval shield, five feet long, and two and a half wide, formed by extending gauze over a thin frame of iron. The metallic gauze is of iron, and the intervals between the threads about one-twenty-fifth of an inch each.

When at Geneva, M. Aldini instructed the firemen in the defensive power of his arrangements, and then practised them, before he made the public experiments. He shewed them that a finger enveloped first in asbestos, and then in a double case of wire gauze, might be held in the flame of a spirit-lamp or candle for a long time, before inconvenient heat was felt; and then clothing them, gradually accustomed them to the fiercest flames.

The following are some of the public trials made. A fireman having his hand inclosed in a double asbestos glove, and guarded in the palm by a piece of asbestos cloth, laid hold of a large piece of red hot iron, carried it slowly to the distance of 150 feet, then set straw on fire by it, and immediately brought it back to the furnace. The hand was not at all injured in the experiment.

The second experiment related to the defence of the head, the eyes, and the lungs. The fireman put on only the asbestos and wire gauze cap, and the cuirass, and held the shield before his breast. A fire of shavings was then lighted, and sustained in a very large raised chaffing-dish, and the fireman approaching it, plunged his head into the middle of the flames, with his face towards the fuel, and in that way went several times round the chaffing-dish, and for a period above a minute in duration. The experiment was made several times, and those who made it said they suffered no oppression or inconvenience in the act of respiration.

The third experiment was with the complete apparatus. Two rows of faggots, mingled with straw, were arranged vertically against bars of iron, so as to form a passage between, thirty feet long, and six feet wide. Four such arrangements were made, differing in the proportion of wood and straw, and one was with straw alone. Fire was then applied to one of these double piles; and a fireman, invested in the defensive clothing, and guarded by the shield, entered between the double hedge of flames, and traversed the alley several times. The flames rose ten feet in height, and joined over his head. Each passage was made slowly, and occupied from twelve to fifteen seconds; they were repeated six or eight times, and even oftener, in succession, and the firemen were exposed to the almost constant action of the flames for the period of a minute and a half, or two minutes, and even more.

When the course was made between the double range of faggots without straw, the fireman carried a kind of pannier on his back, prepared in such a way as to be fire-proof, in which was placed a child, with its head covered by an asbestos bonnet, and additionally protected by the wire-gauze shield.

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Four firemen made these experiments, and they agreed in saying, that they felt no difficulty in respiring. A very abundant perspiration came on in consequence of the high temperature to which they had been exposed, but no lesion of the skin took place except in one instance, where the man had neglected to secure his neck by fastening the asbestos mask to the body dress.

No one present could resist the striking evidence of defence afforded when they saw the armed man traversing the undulating flames, frequently hidden altogether from view by them as they gathered around him.

The fact that in M. Aldini's apparatus a man may respire in the middle of the flames is very remarkable. It has often been proved, by anatomical examination, that in cases of fire many persons have died altogether from lesions of the organs of respiration. It would appear that the triple metallic tissue takes so much of the caloric from the air as it passes to the lungs, as to render its temperature supportable; and it is known, by experiments in furnaces, that a man can respire air at 120 or 130° C. and even higher. Perhaps also the lesions referred to may have been due to aqueous vapour, which is often produced in great abundance in fires where endeavours are made to extinguish them by water, for such vapour would transfer far more heat to the lungs than mere air. Hence in every case, and however guarded, firemen should enter houses in flames with great prudence, because the circumstances are not the same as in the experiments just described.

It is remarked that several suits of this defensive clothing should be provided, not to clothe many persons at once, but that, in endeavouring to save persons or valuable things in cases of fire, the fireman should not approach again and again in heated clothing, but have a change at hand. The Grand Duke of Tuscany has ordered six suits for the city of Florence.

M. Aldini shewed several experiments relative to the extinguishing power of his preparations before the Société de Physique de Genève. One consisted in placing an asbestos cloth of loose texture over a flame either of wax or alcohol; the flame was intercepted as well as it could have been by a piece of wire gauze. This experiment is supposed to favour the objections made to Sir H. Davy's explication of the theory of the wire gauze safety-lamp; but there seems to be a mistake in the idea which has been taken of that theory. Sir H. Davy never explained the effect of his lamp by absorption of heat from flame dependant upon the good conducting power of the tissue alone, but by the joint action of absorption and radiation. There is no doubt that cloth of asbestos is an admirable radiator, and that this power, with its conduction, is probably sufficient to explain the effects upon Sir H. Davy's theory.—xli. p. 333.

13. *Astronomy.*—It was mentioned in a former number of this

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Journal, that a plan for a minute survey of the heavens had been proposed by, and partially executed, under the superintendence of the Royal Academy of Sciences of Berlin. The nature of this plan we previously detailed; and stated at the time, that two maps, one by M. Inghirami of Florence, the other by M. Harding of Gottingen, executed in accordance with it, had been delivered to the Academy. Since then, a third map has been contributed by a countryman of our own, the Rev. T. Hussey, with the assistance and co-operation of the Rev. F. Dawson. Relative to this work, which required three years for its completion, the following letter, addressed to the former of these gentlemen. by M. Encke, secretary to the Royal Academy of Sciences of Berlin, has been communicated to the Astronomical Society of London:—

"You have doubtless, Sir, been surprised at not receiving any earlier information of the arrival of the map and catalogue of stars which you delivered to the Prussian ambassador in July last. You will excuse me, however, on my assuring you, that the parcel did not reach my hands till the 22d of October. This great delay has, fortunately, not occasioned injury to the drawing you so zealously executed. The whole was in very good condition. It is at present at Konigsberg, whither I sent it immediately after having compared the sixty squares, which are common to yourself and M. Harding, from whom we have been so fortunate as to receive the XVth hour.

"The agreement of the observed stars is almost perfect. As to those which are added, it appears, that the faintness of their light has not permitted so exact an accordance. Sometimes M. Harding has thought right to insert a few points which are not to be found in your map, sometimes the latter contains a few more than M. Harding's map. I hope it will soon be in my power to inform you of the opinion of my colleagues, both as to this map and to your obliging offer, to undertake another part of the heavens, if that should be necessary for the completion of the work—but I am unable to anticipate the decision of the committee. I would not longer delay removing your anxiety as to whether a work on which you had bestowed so much valuable time had reached us safely. Requesting you to accept the assurance of my high esteem,

"I have the honour, Sir, &c.

Berlin, November 4, 1829. "ENCKE."

When the variable nature of this climate is considered, and the particularly unfavourable state of the weather during these last two seasons, when the XIVth hour was above the horizon, we are by no means surprised, that a discrepancy should exist in inserting in a map stars below the ninth magnitude, when, moreover, those 15° south of the equator, did not attain at Chislehurst, where the comparison between the map and the heavens was instituted, a sufficient elevation to be free from the vapours of the horizon itself.

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14. *Achromatic Spectacles.*—In our last number we mentioned a very ingenious artificial horizon, executed by Mr. Newman, of Regent-Street, and passed a well-merited eulogy upon his instruments in general. We have recently been called upon to examine some achromatic spectacles, which he has constructed especially for the use of the medical department in the West Indies. For persons requiring glasses of from four down to one inch focal length, and such are all who have undergone the operation of couching, these achromatic spectacles are invaluable: not only is *all* false light gotten rid of, but the extent of field that is gained would not be credited, except by those who have tried them. Where lenses of greater focal length than four inches are employed, the resulting advantage is but trifling. We feel pleasure in noticing this improvement made by a respectable and deserving artist.

15. *Economical process for imitating Silver Paper.*—The following Chinese process has been made known in Europe by Père Du Halde. Take two scruples of gelatine, or Flanders glue, made of ox hide, one scruple of alum, a pint of water. Place the whole over a slow fire until the water is almost entirely evaporated; spread some sheets of paper upon a table, and with a brush lay on two or three coats of this glue; then take a powder made of a certain quantity of talc boiled, and one-third of the same quantity of alum. After having well pounded these substances, sift them, then boil them again in water, then dry them in the sun and pound them again. The powder, which is then very fine, is to be passed through a very fine sieve upon the sheets of prepared paper. The talc powder is glued fast; it is then to be dried in the shade; after which, remove the superfluous powder with a piece of cotton.—*Journal des Connaiss. Usuelles.*

16. *On the Magnetic Influence of the Violet Ray, by* Professor Zantedeschi, of Pavia.—When Professor Morichini published in 1812 his experiments on the magnetic influence of the violet ray, there was no natural philosopher in Europe who did not wish to repeat and vary them; but, unluckily, the attempts of the most skilful men were not always attended by the success they had a right to expect. Thus it is not surprising that many of the most eminent savans doubted the results of the Italian professor. It was only in 1826 that Mrs. Somerville confirmed, by the most decisive experiments, the fact which he had advanced, that the violet ray possessed a magnetic property. Nevertheless, philosophers were not, as yet, satisfied; they could not verify when they pleased the results obtained, nor discover the causes which impeded the success of their experiments. This state of things led me to undertake a series of experiments on the subject, in this very town, where Professor Configliacchi, in the year 1813, had already made some remarkable attempts. It was only with great distrust that I under-

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took anew, a work which this savant appeared to me to have completed with admirable sagacity; nevertheless, the success of my researches has exceeded what I could have hoped. I shall briefly state the method which I followed, and the causes which may prevent the magnetization which it is the object to produce.

As for the method, I introduce into a dark room a solar ray, by means of an heliostat, and decompose it, so that the spectrum is formed horizontally; I then place under the violet ray, in a direction perpendicular to that of the magnetic meridian, the extremity only of the wire that I wish to magnetize. In this way I obtained the following results:—

i. Having placed in the above position a wire of soft iron highly polished, four inches in length, and a quarter of a line in diameter, at the end of 5 minutes, I found that the extremity exposed to the violet ray had acquired a north pole. At the end of 8 minutes, this wire, presented to a magnetised needle, shewed very decidedly that it had poles.

ii. I exposed, in the same way, two wires of soft iron, similar to the preceding, to the action of white light; at the end of 5 minutes each of the two exposed extremities had acquired a north pole; but it was weak, and at the end of a few minutes had disappeared. In the first case, as in the present one, I ascertained, carefully, that the wires employed did not previously possess any sensible magnetism.

iii. The violet ray reversed the very decided poles of a wire of soft iron; it developed, very distinctly, in 6 or 7 minutes, those of another wire, which evinced at its two extremities a very feeble repulsion for the pole of a needle.

iv. Having placed one extremity of a magnetized needle in the red, orange, yellow and green rays, and having observed the nature of its poles and their energy, at the end of 6 or 7 minutes I found no alteration whatever; neither did I remark any effect produced by this operation on a needle which had no perceptible magnetism.

v. A soft iron wire, covered with a coat of rust, and strongly magnetized, having been exposed to the violet ray, in 8 minutes the south pole was converted into a north pole.

vi. A wire of soft iron, highly polished and magnetized, having been exposed by its two extremities to the violet ray, in 10 minutes a north pole was formed at each of its ends.

vii. If the iron wire be oxidized, the same effect takes place in 5 minutes.

The dimensions of all the wires employed was uniformly the same as those of the one that has been mentioned. All these experiments, which, repeated several times, invariably gave the same results, have placed beyond doubt the magnetizing property of the violet ray; but I must add, that in performing them I have encountered innumerable difficulties, which have clearly shewn to me the causes of the bad success of the attempts of many philosophers. I

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shall not here detail the facts which have led me to general results, because that would lead to too great length, and no advantage to science would result from it, but I shall merely point out the following observations:—

i. The wire made of iron from a sulphurous mine cannot be magnetized; it is the same with iron that has been highly tempered; to which, however, I have occasionally succeeded in communicating some slight degree of magnetism.

ii. At low, or at least not high temperatures, such as − 6° R. 0°, and + 10°, only an equivocal magnetization is obtained; and it is in vain to attempt to reverse the poles of a magnetized wire, which I learned from a very long series of experiments made during last winter. While by experimenting at a temperature of + 20° Cent. as Mrs. Somerville did, or at + 25°. or 26° R. as I myself did during the June and the July of last year, very remarkable results are obtained.

iii. Rather thick wires acquire a perceptible magnetism, but with difficulty.

iv. By passing the violet ray from the middle to the extremity of the needle, only weak and uncertain, if any, results are obtained.

I shall finish this note by proposing to examine if the action of the violet ray be not a chemical action. At first this action may be attributed to the feeble currents which take place between the red ray and the violet ray, and the existence of which I have several times ascertained by means of a multiplier suitably arranged; but if such were the cause of the phenomenon, the part of the wire placed under the violet ray ought, as it is evident, to acquire a south pole: now it has been seen that it constantly acquires a north pole.

It may also be thought that the magnetization in question is due to the difference of temperature of the several parts of the wire; but then, again, according to the thermoelectric law, it is the south pole which should be developed in the end exposed to the violet ray. Beside, in this case, where the temperature is the same throughout the whole extent of the wire, no magnetization should take place; now, we have seen (vi.), that a north pole is then formed at each extremity. Lastly, I shall add, that having produced artificially a lower temperature than that of the surrounding atmosphere, I observed the same effects as before, although in a less degree of intensity. These considerations lead me to think that the violet ray acts chemically. I am confirmed in this opinion, by seeing that the carburets and not the sulphurets of iron can acquire magnetic properties; that the needles artificially oxidized, present the phenomenon in question more quickly and to a greater degree than those which are not so; and that, according to the degree of temperature, the magnetic influence of the violet ray increases, becomes weaker, and evanescent. To extend my views on this

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point, I wished to see if I should obtain analogous effects from the light of a candle and from that of the moon. At the end of three-quarters of an hour I obtained a slight degree of magnetism in a needle exposed to the violet ray from the light of a candle; but that from the moon had no effect. I must say, that when I experimented upon the lunar ray, the temperature did not exceed + 5° R. When I shall have repeated the experiment in another season, I will publish the result.

From what is above stated, I am convinced that philosophers operating in the way pointed out will find magnetism developed by the ray, and for which neither the climate of Italy nor that of England is required, but only the precautions stated above. They will also perceive that the magnetism thus obtained is not temporary but permanent, as I have ascertained, by finding that at the end of eight months my wires and needles were still magnetic.—*Pavia, April* 10, 1829.—*Bibl. de Genève.*

17.—*Dr. Arnott's Natural Philosophy.*—(To the Editor of the Quarterly Journal.)

"Sir,—The public attention has been arrested by the statement in Dr. Arnott's new work on heat, that power is derivable from expanded air, at one fourth of the expense by which it is obtained from the conversion of water into steam.

"Dr. Arnott's inference is founded on an estimate of the quantity of heat absorbed in the production of one cubic foot of steam, and of that required to double the elastic force of five cubic feet of air. The result is deduced from the ordinary tables of specific and latent heats, and of the consequent developement of elasticity. The calculations are, I believe, correct as far as they go; but they neglect, I think, one most important consideration.

"It is proposed to heat a certain quantity of air in a certain vessel, from common temperature up to 500°, and this air so heated and expanded, having done its work, is to be exchanged for a fresh supply of cold air, to undergo the same process. But the vessel in which the air was heated to 500° will be itself equally heated, and will communicate the heat to the fresh entering air; it follows, therefore, that a full supply of cold dense air will not enter spontaneously as Dr. Arnott assumes; it must be forced in by a pump, or the vessel must be allowed to cool; if the pump be employed, the power required to fill the vessel will be at least one-half of that derived from the expansion; if the cooling process be adopted, we revert to the systems of Savery and Newcomen, in which the evaporation of a few ounces of water was accompanied by the heating and cooling of some hundred pounds of metal.

"No difficulty of this sort exists in the modern steam-engine, owing to that valuable property which allows matter in two different states, and requiring enormously different quantities of heat, to co-exist in the same place. Two contiguous quantities of differ-

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ently heated matter in the same state, whether solid, liquid, or gaseous, quickly exchange temperatures. But the steam and water in an engine-boiler, though containing very different quantities of heat, remain in contact without any such tendency. Water can, therefore, be supplied to the vessel in which it is to be evaporated, before it can expand into steam, which would oppose its own entrance; while a quantity of already existing vapour could not be introduced without great difficulty. Even when the supply of water requires the aid of a force-pump, as in high-pressure engines, the water (having a bulk five hundred or a thousand times less than that of the vapour it is to form) demands a very small expenditure of power, as compared with the introduction of one measure of gas, which is to be expanded only into two.

"These remarks are offered with great deference to the author of the best epitome of natural philosophy in the English language. But the subject of steam-power is just now so exciting, and the alleged possibility of increasing fourfold the resources of Great Britain is so startling, that you will perhaps think this hasty criticism may tend to elicit a confirmation of what would be an inestimable idea, or else to guard against the ruinous attempts which might be induced by a flattering but delusive speculation.

"I am, Sir, your obedient servant, A. A."

18. *Cement from Iron Filings.* By M. Miallre.—Having reflected about a year since upon the action of vinegar in the preparation of the cement known as *mastic de limaille*, which is prepared in this way—iron filings, garlic, and vinegar, of each a sufficient quantity to form a mass of moderate consistency; I proposed to substitute sulphuric acid diluted with water, for the vinegar, in the proportion of one ounce of acid to a litre (a little above two pints) of water, and to reject the garlic as useless. This alteration was soon adopted by all those to whom I communicated it; for vinegar generally costs in Paris eight or [illegible]ous the litre, while the price of the acidulated water does [illegible]unt to as many centimes. Thus an architect, to whom I [illegible]t known, assures me that this change, which at first appe[illegible]rit no attention, will occasion a saving in Paris alone of [illegible]10,000 francs annually, and therefore deserves to be [illegible]sively known. This cement is generally employed to clos[illegible] of the stones with which most terraces are covered, &c[illegible]t takes place in this operation, it is easy to see—the iro[illegible]h which the [illegible] are filled [illegible]cupying a larger spac[illegible]on as they [illegible]e oxidiz[illegible]idation being facilit[illegible] action of [illegible]id with v[illegible]re impregnated, the [illegible]e exactly [illegible].—*Journ*[illegible]*cie—Août.*

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§ II. Chemical Science.

1. *On the Specific Heat of Elastic Fluids.*—This subject, which has been under investigation at various times by MM. Laroche and Berard, Haycraft, De la Rive, and Marcet, has been taken up by M. Dulong, who has applied to it a new method of investigation dependent upon the velocity of sound in the different gases. La Place shewed that the velocity of the sound in air or other elastic media, was importantly influenced and increased above the expected velocity by the heat evolved, as the vibrations producing sound passed through the air; and M. Dulong, by examining and comparing the sounds produced by different gases, has endeavoured to ascertain whether this element is the same in all of them. He arrives at this general law, remarkable for its simplicity, i. That equal volumes of all elastic fluids taken at the same temperature and pressure, when compressed or expanded suddenly by a fraction of their volume, disengage or absorb the same *absolute quantity of heat.* ii. That the variations of *temperature* which result, are in the inverse ratio of the specific heat of a *constant volume.*—*Ann. de Chimie*, xli. 113.

2. *Supposed Influence of Magnetism over Chemical or Crystallizing Powers.*—The investigation of this influence, which has been repeatedly asserted and denied, has been undertaken in a very careful and particular manner by Professor Erdmann. He first points out the number of delicate perturbing causes which may and have occasionally led to mistakes, pointing out the effects produced by irregularity in the wires—handling them with the uncovered fingers, &c. &c.; and especially states that many repetitions of each experiment should be made. The bars and magnets which he had occasion to use were very powerful, some of them competent to lift 80 pounds.

i. In experiments made to ascertain the oxidation of iron wire, even under the influence of terrestrial magnetism, it was ultimately proved, i. That the oxidation of iron placed under water is not at all influenced by terrestrial magnetism; there is no point of the horizon towards which it is more strongly or quickly produced than towards another. ii. The oxidation arising from unequal contexture of the iron always begins at the points where the wire is in contact with other bodies, not only metals, but even wax or baked earth. iii. Diffuse daylight, or the weakened rays of a winter's sun, neither retard nor assist oxidation, provided they are accompanied by no change of temperature.

ii. In experiments made with magnetized wires the results were the same; no difference of oxidation occurred at the two poles or other parts.

iii. In experiments on the reduction of metals by the humid pro-

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cess, as in the *arbor Dianæ* *, no influence of terrestrial magnetism could be observed. The crystallization took place in the branches of the syphon tube, and without any reference to their direction.

iv. In repeating the experiments with the additional power of a very large magnet, its poles proved to have not the slightest power over the formation or disposition of the crystal within.

v. Numerous salts were made to crystallize slowly in vessels placed over the poles of magnets, with every care that their power as conductors of heat should not interfere. The magnetism exerted not the slightest influence over the crystallizations. In chemical actions, where gas was evolved, no difference in the rapidity of evolution, or quantity of gas produced, occurred, when magnets were present or absent.

vi. No evidence of the influence of the magnetic poles over the colours of vegetable solutions could be obtained.—*Bib. Univ.* xlii. 96.

3. *On Phosphoric Acid, by* M. Gay Lussac.—Engelhart observed, that recently fused phosphoric acid, when dissolved in water, precipitated albumen, although it did not do so before fusion, and lost the property when the solution had been preserved some time. More lately Mr. Clarke discovered, that phosphate of soda, exposed to a red heat, acquired properties different from those it had before calcination. It becomes less soluble, contains less water of crystallization, is changed in form, and precipitates nitrate of silver, white instead of yellow.

These observations of Engelhart and Clarke appearing to me to have some analogy, I made certain experiments. I took liquid phosphoric acid, which had been a long while in the laboratory, and after ascertaining that it did not precipitate albumen, I saturated it with carbonate of soda. The phosphate produced, precipitated nitrate of silver yellow. Another portion of the same acid calcined, and then saturated by soda, precipitated nitrate of silver white.

Calcined phosphate of soda was then decomposed by nitrate of lead, and the phosphate of lead formed decomposed by sulphuretted hydrogen. The phosphoric acid thus produced precipitated albumen, and, when recombined with soda, precipitated nitrate of silver white.

From these observations it appears, that the remarkable alteration in properties observed by Mr. Clarke, in calcined phosphate of soda, is due to that which phosphoric acid undergoes in the same circumstances. This is still further proved by the circumstance that the phosphates of potash and ammonia made with calcined phosphoric acid, precipitate the nitrate of silver white, and that ordinary phosphate of potash acquires the same power by calcination.

* *Quarterly Journal*, N. S. iv. p. 429.

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According to these results, which I have not had time to multiply, Mr. Clarke's opinion of the cause requires to be modified. They are sufficient, however, to make us conclude that very remarkable differences exist between most phosphates before and after calcination, or between those made with calcined or uncalcined acid. It is to be remarked, that the modification of phosphoric acid by heat is much more permanent when it is combined with a base than when in aqueous solution. I hope to give other details on this subject at a future period.—*Annales de Chimie*, xli. 331.

4. *Production of Sulphuric Acid from the Vapours of the Aix Waters.*—The mineral hot waters of Aix, in Savoy, are two in number, and are distinguished, one as the aluminous, the other as the sulphuretted. It has frequently been stated that the vapours of the former contained free sulphuric acid. That they produce sulphuric acid is proved by the following observations of M. Francœur:—i. All the grottos, closed chambers, corridors, &c. where the vapours penetrate, have their walls corroded and covered with acid crystals of sulphate of lime. ii. All iron utensils, &c. are not only corroded, but often found incrusted with sulphates of iron and lime. iii. Tubes of cloth, through which the vapours are passed, are quickly rotted, and the rags are found impregnated with sulphuric acid.

No free sulphuric acid exists in the water itself; and from the circumstances it follows, that the mixture arising as vapour, and containing hydrogen, azote, sulphur, and carbonic acid, has the power of producing sulphuric acid by means of the atmosphere and in contact with the walls and metal.—*Ann. des Mines*, v. 285.

5. *Carburet of Sulphur decomposed by Voltaism.*—M. Becquerel has described an experiment on the decomposition of carburet of sulphur by low voltaic powers. Liquid carburet of sulphur was put into a tube (glass), and above it a solution of nitrate of copper: then a plate of copper was plunged into and left in both solutions. This formed a feeble voltaic pile, and chemical decomposition took place. Both the carburet and the nitrate were decomposed, much crystallized protoxide of copper was formed on the copper plate, and carbon was deposited on the sides of the tube, in very thin plates, with a metallic lustre. No formation of diamond.—*Revue Encyc.* xliii. 508.

6. *Composition of the Atmosphere at Kazan.*—The uniformity of the composition of the atmosphere taken from different places and heights upon the earth's surface is well known; but M. Kupffer still thought it advisable, when he had the opportunity, to analyse a portion of air collected at Kazan; a place surrounded on the one side by a poorly cultivated country, and on the other by the immense steppes and forests of Siberia, where vegetation is dormant for the greater part of the year. He used Volta's eudiometer, and still

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found that the air contained from 21 to 21.2 per cent. of oxygen.—*Annales de Chimie*, xli. 423.

7. *Disinfecting powers of Chloride of Lime.*—M. Poutet, of Marseilles, says, that this substance cannot be used with advantage in destroying the bad odour of fish or marine animals, for that it evolves one as bad as any they can previously possess. The powder, added with a little water, to fresh or salt fish, cut into small pieces, evolved such an odour of bromine as to be insupportable. The muscle of putrid fish produced a still worse smell; and the same thing took place with other marine products, as shell-fish, sponges, &c. &c.

8. *Proportions in which Oil Gas and Air detonate.*—The oil gas experimented upon by M. Dumas, contained 18 per cent. of vapours, absorbable by sulphuric acid in a few minutes; 100 parts required 270 of oxygen for complete combustion, and produced 174 parts of carbonic acid gas. The combustion was made in a detonating Volta's eudiometer, by means of a strong spark from a Leyden jar; or on those mixtures near the limits of combustibility by a series of sparks. One volume of gas with

Air.	
1, 4, 6, and 7,	no inflammation.
8,	detonation—fuliginous flame.
9,	detonation strong—no smoke.
10 and 11,	very strong detonation—*maximum*.
12,	detonation less strong.
13,	detonation still less.
17,	feeble detonation.
18,	very feeble detonation.
20,	feeble detonation at the second spark.
21,	——————— after many sparks.
25,	no detonation after many sparks.

These results were in winter at the temperature of 40° or 42° F.—*Ann. de l'Industrie.*

9. *On the proportional Number of Lithium.*—Arfwedson, the discoverer, gives this number as 255.63 (Berzelius scale), Gmelin as 191.21 only, and Kralovansky as 254.2. This difference has induced M. Hermann, who possessed a considerable quantity of it, to examine the subject minutely. His lithia was procured from the mica of Altenberg, in Saxony, at first in the form of double phosphate of soda and lithia, which was converted into chloride of lithium, and then by Berzelius' process into carbonate of lithia, by means of excess of carbonate of ammonia. The *carbonate of lithia* thus formed was heated to redness, and found to fuse into a clear liquid, strongly acting upon platina. After cooling, it became diaphanous and crystalline, and easily broke into numerous fragments. Being analysed by Murschand, it gave 39.02 lithia and 60.98 carbonic

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acid, a composition nearly the same with that of the carbonate obtained when precipitated from the muriate by carbonate of soda. Hence the lithia was proved to be free from soda, and to have 352.06 as its number.

Sulphate of lithia, formed from the carbonate and sulphuric acid, was readily crystallizable by slow evaporation. It is freely soluble in water, but not more in cold than hot. The crystals are oblique quadrangular prisms, containing 85.70 of dry salt and 15.3 of water per cent.; 100 parts of the dry salt, dissolved and decomposed by baryta, gave 74 sulphuric acid and 26 lithia. The equivalent number, according to this analysis, would be 352.1, and that of the metal lithium 152.1.

Muriate of Lithia.—When chloride of lithium is left to deliquesce in the air, large regular crystals are gradually formed of muriate of lithia. They have the singular property of becoming opaque when touched, or put upon a filter, the opacity beginning at the place touched. When the opaque crystal is pressed it falls into small crystalline fragments. The opacity, therefore, arises from a spontaneous division of the large crystals, the cause of which is not yet known. The crystals are right-angled prisms: they consist of 1 atom chloride of lithium and 8 of water, or experimentally 53.64 salt and 46.36 water per cent. When heated, the salt fuses and loses chlorine. This is in consequence of the action of oxygen, and the salt always becomes alkaline.—*Annalen der Physik.*

10. *Adulterations of the Iodide of Potassium.*—The iodide of potassium, or hydriodate of potassa, is so convenient a form of iodine, that it is constantly retained amongst medical preparations. Its utility and consequent value has led, as usual, to adulterations, one of which is described by M. Pereira, where the extraneous matter amounted to 77-hundredths of the whole. In this case the substance added was carbonate of potash; and it may easily be supposed to what an extent errors of judgment may proceed amongst medical men, when such fraudulent mixtures as these are placed in their hands by the dishonest or deceived dealer.

M. Pereira discovered the falsification whilst attempting to make a solution of iodine in the hydriodate of potassa, a preparation kept at the general dispensary. The supposed hydriodate refused to dissolve the quantity of iodine which ought to have been taken up, the carbonate present not having an equivalent power.—*Med. Jour.* lxii. 310.

11. *Preparation of red Ferro-prussiate of Potassa.*—M. Kramer has endeavoured to prepare this useful test by a process more ready, and less troublesome, than that of Gmelin. By decomposing Prussian blue at a low temperature by chloride of potassium, he obtained a liquid of a yellow colour when in small quantities, but red in larger portions; it did not precipitate persalts of iron, but

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did precipitate the protosalts. By evaporation, the solution gave a little Prussian blue, and then crystals, the summits of which were quadrangular pyramids. By two or three crystallizations these were rendered perfectly pure.

The red ferro-prussiate of soda could not be obtained in a similar way, but only by passing chlorine through the ordinary ferro-prussiate of soda, formed by digesting soda on Prussian blue. By a similar process he obtained a red ferro-prussiate of ammonia, but the compound was not permanent.—*Jour. de Pharmacie.*

12. *New Method of analysing Alloys of Copper and Silver.*—This method is proposed by Professor Zenneck, of Stutgard, and is founded upon the constant quantities of hydrogen gas evolved when either silver or copper is dissolved in pure strong muriatic acid. The solution requires to be assisted by heat; and when copper is the metal operated with, the surface of the acid in contact with the air must be covered by a layer of oil; with these precautions, a given weight of copper always gives out a proportionate and equivalent quantity of hydrogen gas, and a given weight of silver also, its determinate equivalent; being in bulk less than that evolved from copper. The difference between the bulks of gas evolved by equal weights of the two metals enables a careful experimenter to determine, from a given weight of alloy and the gas it evolves, the proportions of silver and copper present.

The instrument used by Professor Zenneck is a glass tube closed at one extremity, and bent nearly to a right angle. It is expanded in the angle in the manner well known to chemists, so that when filled with liquid any acid rising from a solid piece of matter in the angle should ascend into the closed branch. It is, in fact, a bent tube pneumatic receiver. The process is fully detailed in M. Zenneck's paper, and also in the abstract given of it in the *Bibliothèque Universelle*, xli. p. 317—but we do not think it necessary to describe more than the principle. There is no doubt the process will succeed in careful hands; but objections to it, as a ready and sure method, quickly arise in the mind.

13. *Artificial Ultramarine.*—By following Gmelin's process*, M. Hermbstadt has obtained the most beautiful ultramarine imaginable. He attributes the success of the operation principally to the care taken to mix the silicate of soda and alumine in as moist a state as possible. If they are too much dried before adding the sulphur, the colour obtained is only a bluish-green.—*Allg. Handl. Zeitung*, 1829.

14. *A new Earth, Thesina.*—This new earth, thesina, is a recent discovery made by Berzelius, and must be distinguished from the sub-

* *Quarterly Journal*, N. S. vol. ir, p. 216.

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stance formerly called Thorina by the same philosopher. A new mineral body, discovered by Esmark, near Brevig, in Norway, was sent to Berzelius for examination. It was compact, black, brittle, and semi-hard, having the vitreous fracture of gadolinite, a specific gravity of 4.8, and producing a dark brown powder. Under the blowpipe it loses water, and becomes yellow. The mineral contains a new earth so like the substance formerly called thorina, as to be at first mistaken for it; when, however, distinguished by experiment, the name thorina was retained.

The new earth is colourless and infusible after ignition, insoluble in all acids except the sulphuric, and not rendered soluble in other acids by calcination with alkali. It is insoluble in caustic potash, but soluble in the carbonate; heat partially precipitates the solution, cold causes re-solution. Its salts have a pure styptic taste: a strong solution of the sulphate becomes a thick mass by boiling, but it is soluble in cold water; this property particularly characterises the new earth. Sulphate of potash produces a precipitate in the saturated solution, which is a double salt, soluble in cold water. This is a character also of the salts of cerium. Ferro-prussiate of potash precipitates it, as it does yttria.

Potassium does not reduce thorina; but the chloride, obtained in the same manner as with alumina, is readily decomposed with a feeble detonation. The product is a pulverulent grey metallic mass, dissolving rapidly in muriatic acid, and but slowly in the nitric and sulphuric acids. Neither water nor alkalies act upon the metal. By friction it acquires lustre. It burns brilliantly in oxygen gas into thorina, without exhibiting fusion. Thorina contains 11.8 per cent. oxygen. The mineral (thorite), from which it was obtained, contains per cent. 57.91 thorina—18.98 silica—9.5 water—3.4 oxide of iron—2.58 lime—2.39 oxide of manganese, and portions of the oxides of uranium, lead, and tin, with traces of potash, soda, magnesia, alumina, &c.—*Hensman's Repertoire.*

15. *Analysis of Siliceous Minerals by Alkaline Carbonates.*—The ready fusion observed by M. Berthier* of many atomic mixtures of salts may be applied to the analysis of siliceous minerals by alkaline carbonates, aided by a spirit lamp. A mixture of five parts of carbonate of potassa and four parts carbonate of soda is so fusible, that between 200 and 300 grains may be rendered perfectly liquid by a spirit-lamp flame. If sand be added to the mixture, there is an effervescence as lively as if acid had been added. This effervescence occasions the expulsion of part of the substance; and by the addition of too much sand, the mass would become too difficult of fusion, unless the sand or mineral had been previously pulverised and mixed with the carbonates. Hence the operation should commence with the mixture of the carbonates and the mineral. In this manner considerable quantities of felspar may be readily de-

* *Quarterly Jour.*, M. S. vol. iv, p. 436.

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composed by the heat of a spirit of wine lamp.—*Annalen der Physik*, 1828.

16. *Gay Lussac on the Action of Potassa on Organic Matters.*—M. Vauquelin, by treating pectic acid with potash in a crucible, converted it into oxalate of potassa. This experiment suggested to me the idea of submitting ligneous matter, which has an analogy to pectic acid, to the same treatment, and I obtained the result I expected.

5 grains of cotton were put with 25 grains of pure potash and a little water, into a platina crucible, and heated over a spirit lamp much beneath redness. The cotton resisted the action of the alkali at first, but ultimately softened, the mixture melted without undergoing carbonization, and hydrogen was disengaged. During the tumefaction the mixture should be continually stirred. When it had settled down, the mass was dissolved in water, and rendered slightly acid by nitric acid. It then gave an abundant precipitate with nitrate of lead, and this, operated upon by sulphuretted hydrogen, produced very fine crystals of oxalic acid. With nitrate of lime, a voluminous precipitate of oxalate of lime was obtained.

Wood sawdust, with the same treatment, gave the same result.

Sugar, with 4 or 5 times its weight of potash, became, when heated, at first brown, then white, and gave much oxalic acid.

Starch formed a very glutinous mass with potash, which long retained this state. More alkali occasioned liquefaction, the mixture swelled, and oxalate of potash was produced.

Gum and sugar of milk were also converted into oxalic acid with the disengagement of hydrogen.

The most remarkable transformation is that of tartaric acid into oxalic acid. There is no swelling, no blackening, and (which merits particular attention) so little evolution of hydrogen, that it may be considered as due to extraneous vegetable matter. When the hydrogen is to be collected, the experiment may be made in a retort to which a tube of glass has been attached, which is to be plunged beneath a layer of water into mercury to prevent absorption. The retort being heated by a bath of mercury or oil, it will be readily observed that a temperature of 400° F. at most is sufficient to form the oxalic acid.

Citric and mucic acid produced also much oxalic acid. I have also obtained it from succinic acid, but the benzoic acid resisted the action of the potassa, and remained unaltered.

Acetate of potash heated with excess of potash became converted into carbonate. A little oxalic acid was obtained, but it is very probable that it was due to extraneous vegetable matter.

Colza oil, notwithstanding a great excess of potash, could not be brought into fusion, and but very little oxalic acid was obtained.

Amongst animal substances, silk, treated with potash, gave oxalic acid with disengagement of hydrogen. Uric acid evolved ammonia.

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The residue was very white; being dissolved in water, and saturated by nitric acid, hydrocyanic and carbonic acid were disengaged; nitrate of lime then produced an abundant precipitate of oxalate of lime. Gelatine gave a similar result. Indigo gave no oxalic acid.

Carbonate of potash used instead of caustic potash gave no oxalic from tartar; nor did lime and starch produce any oxalic acid. Soda may be used instead of potash.

From these experiments, it appears that a great number of animal and vegetable substances, acted upon by caustic potash or soda, are transformed into oxalic acid. It is to be remarked that the formation of this acid precedes that of carbonic acid, and precisely under the same circumstances, as when sulphur and potash, for instance, produce hyposulphurous and sulphuric acids. Thus a vegetable substance, heated moderately with potash, gives oxalic acid, but when more strongly heated, carbonic acid.

As very different substances produce oxalic acid, it is necessary that other products should be formed. Many bodies evolve hydrogen which may come from themselves, or from the water they contain, and afterwards carbonic acid. Animal matters, besides these two products, give also ammonia and cyanogen. Water must also be formed with both animal and vegetable substances. These products, or some of them, are sufficient to explain in general the formation of oxalic acid, but in some particular cases there ought to be other products obtained. Thus tartaric acid gives no sensible portion of hydrogen, and yet its composition being $2\frac{1}{2}$ proportions of hydrogen, 4 of carbon, and 5 of oxygen, we cannot explain its transformation into oxalic acid by the occurrence of any of the known products.

In fact, during the operation, the mixture remains perfectly white. If all the carbon entered into the oxalic acid, it would require 6 proportions of oxygen, and consequently water ought to be decomposed to furnish 1 proportion. If only so much oxalic acid were formed as is proportional to the oxygen in the tartaric acid, then $\frac{1}{4}$ of a proportion of carbon would remain, which might form a particular compound with the hydrogen; and for 1 proportion of tartaric acid, $1\frac{1}{4}$ of oxalic acid would be produced. In place of this last quantity, I have obtained $1\frac{1}{4}$ oxalic acid, but I have not discovered any hydrogenated compound. Finally, it was possible that a peculiar acid had been formed by the carbon, oxygen, and hydrogen. This point deserves particular examination, and I should have undertaken it in the vacations, if I had had time, but hope to resume the subject shortly.

I shall conclude by describing a very elegant method of transforming tartar into oxalate of potassa. It consists in dissolving rough tartar in water, with a proper quantity of potash or soda, and making the solution pass by means of a pump in a continual current through a thick tube of iron or bronze heated to 400° or 450° F.

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The pressure need not be more than 25 atmospheres, for no gas will be disengaged. A valve is to be placed at the opposite extremity to that at which the solution enters, and charged with sufficient weight to obtain this pressure; it will then only be opened by the pressure exerted by the injection pump. I have not as yet tried this process, which is also applicable to other substances, but I see nothing which can prevent its success. According to some experiments which I have made, less than a proportion of potassa for a proportion of neutral tartar will be necessary.—*Ann. de Chimie*, xli. 398.

17. *New Source of Spirit.*—It is stated that the berries of the *Sorbus Aucuparia* are now used in the north of France for the production of a spirit, and the result is said to be equal to the purest distillation from grapes for brandy. The berries, when perfectly ripe, are first exposed to the action of cold in the open air, then put into a wooden vessel, bruised, and boiling water poured on, the whole being stirred until it has sunk in temperature to 82° F. A proper quantity of yeast is then added, the whole covered up and left to ferment. When the fermentation is over, the liquor is to be put into the still, and drawn over in the usual way. The first running is weak and disagreeable in flavour, but being distilled from off very fresh finely powdered charcoal in the proportion of 8 or 9 lbs. to 40 gallons of weak spirit, a very fine product is obtained. The charcoal should remain in the liquid two or three days before the second distillation.

18. *Effect of Ether on Sulphate of Indigo.*—M. Cassola states, that when sulphuric ether is added to sulphate of indigo, in about half an hour, at the temperature of 100° F., the colour of the indigo totally disappears, and no substance whatever is capable of restoring it. The colourless mixture being subjected to distillation, yields a liquor which reddened litmus strongly, and gave no precipitate with barytic salts; but with nitrate of silver a precipitate was obtained soluble in ammonia.—*Henman's Rep. Phil. Mag.* N. S. vi. 393.

19. *Composition of Malic Acid.*—M. Frommherz has endeavoured to settle the discordances in the experimental estimation of the proportions of carbon, oxygen, and hydrogen, in malic acid. He burnt the malate of lead by peroxide of copper. His malate consisted of 39.375 malic acid, and 60.625 oxide of lead. From the mean of three experiments, he deduced that the acid contained 6 atoms of oxygen, 7 atoms of hydrogen, and 3½ atoms of carbon, or

Carbon	29.357
Oxygen	65.863
Hydrogen	4.780

Then it is remarked that a half atom is contrary to all theoretical

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ideas, and *therefore* that malic acid must contain just double the number of atoms given above, or else that carbonic acid is formed of an atom of carbon and an atom of oxygen. We recommend the facts as useful; the theory merely as amusement. What is an atom!

20. *Chemical Constitution of Acetic Ether.*—By a series of experimental researches, M. Planiava has arrived at the conclusion, that acetic ether is formed of 1 equivalent of acetic acid and 2 equivalents of alcohol; that, therefore, it is a sub-acetate of alcohol, and is represented by the number 97.—*Kas. Archives.*

21. *New Vegeto-Alkalies obtained from Cinchona.*—Dr. Serturner, in re-examining the products obtained by chemical means from the cinchonas, finds that the precipitates produced by alkalies from the acidulated infusion of these barks contains, besides cinchona and quinia, other vegeto-alkalies, which are to be considered as modifications of the former. The new bodies recall the case of opium to mind, in which narcotine exists simultaneously with morphia. The new substances, and especially that named by M. Serturner *chinioïdia*, exist in the alkaline precipitate, in intimate combination with a resinous subacid substance, which is not injurious, but is of no advantage. It is very difficult to separate these two substances, and M. Serturner succeeded only when he used the charcoal obtained when croconic acid is prepared by Liebeg's process. This substance, combined with animal charcoal, completely decolours the solution of the alkaline matter in sulphuric acid (diluted with 3 or 4 parts of water), but it is necessary afterwards to act on the thick solution with alcohol, to separate earthy salts.

The new vegeto-alkalies exist in the red and yellow cinchona with the quinia and cinchonia. The chinioïdia has more alkaline power and capacity of saturation, and also more medical power than any other vegeto-alkali in the cinchona, but it resembles them by its insolubility in water, its colour and taste. Its alkaline reaction on known vegetable colours, and its intimate state of combination with the brown extractive matter, are remarkable. Its salts are very fusible by heat, and become viscid like some balsams.

According to M. Serturner, in febrifuge power, chinioïdia is as superior to quinia and cinchonia as these are to ordinary bark. It is to this alkali that many cinchonas are indebted for their medical powers. M. Serturner has, in many cases, given his new medicine in doses of 2 grains three times per day; the patients take a little vinegar after each dose, for the purpose of saturating the gastric juice, which, by its alkaline nature, would else decompose the salt: from 12 to 24 grains have, in all the cases, sufficed to prevent the return of the fever, whilst patients, in the same neighbourhood, treated with the sulphate of quinia, had frequent returns of the disease.—*Hufeland's Journal.*

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22. *Investigation of Tobacco—Nicotia.*—It has been two or three times supposed that a vegeto-alkali has been obtained from tobacco, to which its peculiar properties are due. Lately MM. Posselt and Reimann have resumed the investigation, and think they have obtained the true principle by two processes. i. Mix 1½ lb. of tobacco with 2 ounces of potash and enough water; distil in a glass retort; add fresh water to the residue in the retort, and again distil; repeat this two or three times; after which the leaves will be found deprived of their acrid property, and harmless if taken internally. The united products are to be neutralized by sulphuric acid, and evaporated nearly to dryness; the irregularly crystallized brown mass is to be acted upon by strong alcohol, the solution diluted with a little water, and distilled. The brown aqueous residue is to be treated with concentrated potash, and again distilled, when a clear light-coloured oily substance will be obtained, very acrid, with an odour which, though slight at common temperature, becomes insupportable by elevation of temperature. Ether added to this substance in successive portions takes away the acrid principle, and the solution, when distilled, leaves the pure principle in a very concentrated state. ii. Or boil 12 lb. of dry tobacco leaves in water, acidulated with sulphuric acid, evaporate, and treat the residue with alcohol, diluted with a ninth of water; add a little water to the solution, and distil; add hydrate of lime to the aqueous residue, and redistil; the product being mixed and agitated with ether, the latter is to be poured off, and a fresh portion added as before. All the ethereal solutions are to be conjoined and put in contact with muriate of lime, which will take away the water; and the concentrated ether solution being evaporated or distilled, will leave 3 gros or 118 grains of reddish brown *nicotia*.

Pure nicotia is limpid, and liquid at 21° F.; its odour resembles that of dry tobacco; its taste very acrid, burning, and enduring. It stains paper, but the mark disappears in some hours: it is heavier than water, volatilizes in the air, and boils at 474° F. It burns round a wick, and produces white vapours at 212° F. It dissolves in water in all proportions, and the solution has an alkaline reaction. It dissolves in alcohol; but when this solution is distilled, the nicotia does not pass over. Ether dissolves it in any quantity, but neither does its vapour carry any portion up. Acids take the nicotia from these ethereal solutions, and form salts insoluble in ether. The phosphate of this substance is crystallizable with difficulty, forming a substance having the appearance of cholesterine. The sulphate is uncrystallizable; nitric acid destroys the body in part; oxalic and tartaric acids form crystallizable soluble compounds.—*Geiger's Mag.*

23. *Preparation of Urea, by* M. Henry.—Let a slight excess of the subacetate or the hydrate of lead be added to recent urine; a precipitate will fall which will contain salts formed by the union of the acids in the urine with oxide of lead, and also a combination of

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the mucus and animal matter present with the hydrate or subsalt used.

The clear fluid is to be acted upon by diluted sulphuric acid, added until in slight excess, to separate the lead present; and to act, during the future evaporation, upon the acetates of soda and lime which may be formed. The liquid is again to be freed from the precipitate, and quickly evaporated, animal charcoal being added to it during the ebullition. When clear the fluid is to be strained through a fine cloth, and concentrated to one-third of its bulk; on cooling, it will probably become a yellowish acicular crystalline mass, consisting of much urea and some salts. The crystals, when drained and pressed, are to be added to those produced by evaporating the mother water, also similarly treated; being thus freed from the brown viscid matter which previously accompanied them, they are to be treated with a small quantity of carbonate of soda, to decompose any acetate of lime which may remain, and then are to be digested in alcohol. The solution, filtered and distilled, leaves urea, which may be recrystallized by solution in water and evaporation.—*Journ. de Pharmacie, April*, 1829.

24. *Composition of different Bones.*—The following account of the earthy part of different bones, as given by Dr. F. de Barras, is interesting. The quantities are those obtained from a thousand parts of bone.

	Sheep.	Hens.	Fishes.	Frogs.	Lions.
Carbonate of lime...	193	104	53	24	25
Phosphate of lime...	800	836	919	952	950

—*Jameson's Journal.*

25. *New proximate Principle from Albumen, by* M. Couerbe.—White of egg was left to itself at a temperature of 17° or 18° F. It did not congeal, but thickened a little, and at the end of a month gave an abundant membranous net-work and a liquid matter, no putrid gas having been disengaged. The liquid was slightly examined, and by its decomposition gave carbonate of ammonia; hence it was concluded that the liquid contained the animal part of the albumen.

The membranous substance was white, translucid, and of a foliaceous structure, insipid, inodorous, and friable. Heated in a tube, it did not fuse, but was decomposed, swelling at the time, evolving no azoted products, but leaving a voluminous light charcoal difficult to burn. When decomposed by oxide of copper, it gave only water and carbonic acid. In cold water it did not dissolve; in boiling water it softened, melted, and looked like insoluble mucilage. Alcohol, ether, and acetic acid exerted no action upon it, hot or cold. Sulphuric acid exerted little action at common temperatures; but by heat carbonized it, evolving an agreeable aromatic odour. Nitric acid acted but little when cold; by heat dissolved it, evolving

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nitrous gas. Muriatic acid dissolved it when heat was applied, and the solution remained clear on cooling; by dilution, a fine white powder was deposited.

Potash, with heat, dissolved it, and rendered the solution turbid, but did not cause any deposition in 24 hours.—*Annales de Chimie*, xli. 329.

26. *To ascertain the Admixture of Sulphate of Copper in Bread.*—In the *Observateur de Liège*, MM. Meylinck and Hensmans have given three processes for ascertaining the presence of this salt in bread—the humid way—the dry humid—and the dry. The *dry humid*, which they prefer, consists in letting fall a drop of ferro-prussiate of potassa on a slice of bread; whether or not there is sulphate of copper in the bread, a reddish spot will be formed if the bread be fresh: a blue one, if the bread be sour. Immerse the bread in lime-water, the spot will scarcely change colour, or will become yellow if there be no adulteration; but, on the contrary, will assume a greenish tint if it contain sulphate of copper. In the second case, expose the bread to the action of ammoniacal gas, which will immediately change the blue colour of the spot to a pale citron yellow if the bread is pure; on the contrary, if it be adulterated, the spot first becomes red, then yellow, and from the yellow it may be brought back to the red, by volatilizing the ammonia, or exposing it to muriatic vapour. By the *humid way*, we may proceed either, i. By treating the bread with acidulated water, passing the liquor through a flannel, and then examining it by the ordinary re-agents after having neutralized it by ammonia; or ii. By treating the bread with concentrated alcohol, boiling; the alcohol takes up the salt of copper, and a solution is thus obtained, which is to be treated with the ferro-prussiate. The *dry way* consists in burning the bread in an unvarnished earthen vessel. A stratum of different coloured ashes is thus obtained, covered with an efflorescence of a mixed green and blue colour. According to M. H. it is preferable to treat separately the ashes and the efflorescence; but Dr. Jacquemys, of Liege, regards this as useless, because the copper contained in the whole mass may easily be dissolved in nitric acid. The solution having been effected, it is to be neutralized by ammonia, and enough ammonia added to precipitate the alumina from the flour and dissolved by the acid. The presence of copper may thus be ascertained by the hydroferro-cyanate of potass. To determine the quantity of salt contained in the bread, it is necessary, after having precipitated the alumine, to filter the liquid; add to it prussiate of potass and of iron, then dry and weigh the precipitate, and from this datum calculate the quantity of sulphate of copper.

27.—*Letter from Sig. Carlo Matteucci of Forli to Professor Gazzeri:*—

"Sir,—The importance of the new fact, in solar electricity,

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which I communicated in my letter published in April, in the Anthologia for this year, obliges me to make known, on the same subject, some observations not less interesting than the former. From the time when I first observed this phenomenon, I have adhered to the idea, that the electric state acquired by the glass might depend on the evaporation of the aqueous film, which always covers its surface. Therefore, to remove so doubtful a fact from my observation, I endeavoured to perform the experiment in a different manner. After having frequently touched a flat piece of glass with a trial plane, without perceiving any sensible developement of electricity, I heated it strongly that it might get rid of the moisture adhering to it; and having then left it to get dry under a bell, which had been previously dried by heat and by muriate of lime, I then frequently made trial with the same plane, and never did I see any electricity developed by it, which, however, would have quickly happened if the plate had been abandoned for a few moments to the direct action of the solar rays. Yet, however frequently I saw the evaporation of the stratum of water which covered it, excluded from the causes of rendering electric the plate of glass exposed to the sun's rays, still the touching of the glass plate with the trial plane did not appear to me a method free from all exception, it being too easy to develop electricity either by pressure or by any friction. I wished, therefore, to vary my method of experiment, so as not to have my observation, if correct, lie open to denial. Having added to the condensing plate a metallic wire, soldered at the extremity to a large disc of brass, I placed upon this a glass plate, and made the sun's rays fall upon it, without their touching the box of the electrometer. I then perceived the leaves diverge sensibly, and having raised the plate, and then the collecting plate, I observed, as was very natural, the divergence increase. In this way, the cause of the electric state of the glass exposed to the sun's rays appeared to me reduced exclusively to the power of those rays. If these new statements appear to you interesting to science, they are at your disposal."—*Carlo Matteucci, Forli, August* 13, 1829.

28.—*On a new Oxide of Manganese.* By Mr. Phillips.—Mr. R. Phillips, whilst engaged in examining the oxide of manganese from Warwickshire, was enabled to distinguish certain parts of it from other oxides with which it had been confounded, and to shew that it was a new compound of the metal with oxygen. He has called it *Varvacite*. In general appearance, and many of its properties, it so closely resembles the native peroxide, that there can be little doubt but that it has frequently been mistaken for it.

This mineral has a grey colour, the tint of which is not remarkably different from that of the well-known crystallized peroxide; it is, however, less brilliant. It is much harder than the peroxide, does not soil the fingers so much, and is lighter in the proportion

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of 4.285 to 4.819. When reduced to powder and boiled in water, a trace of muriate of lime is discoverable.

It loses nearly the same by exposure to strong red heat as the peroxide, *i. e.* about 13.26 per cent.; but on examination, it appeared the proportion of oxygen evolved was much less than when peroxide was heated, and the quantity of water more. By close analysis it appeared to consist of,

Manganese	63.
Oxygen	31.6
Water	5.4
	100.0

Or two atoms deutoxide, three atoms peroxide, and one atom water.

Mr. Phillips gives the following view of the known compounds of manganese and oxygen:—

	M. O.	M. O.
Protoxide	1+1	28+ 8
Deutoxide	2+3	28+12
Peroxide	1+2	28+16
Red oxide	3+4	28+10.66
Warwick oxide	4+7	28+14
Manganous acid	1+3	28+24
Manganesic acid	1+4	28+32

So similar is the new compound to some of the old well-known compound of manganese, that at first some difference of opinion as to its nature took place between Mr. Phillips and Dr. Turner; which has, however, been removed by experiments made by the latter, with a true specimen of varvacite.—*Phil. Mag.* N.S., v. 209, vi. 281.

§ III. Natural History.

1. *On the Direction of the Roots and Stems of Plants.*—This subject has been examined closely by M. Poiteau, who, in his Mémoire, considers several facts, and some theories. He first refers to the well-known experiment by Mr. Knight, in which seeds, made to grow on the circumference of a revolving wheel, threw out their radicles in an inclined direction from the centre of motion, and their plumula in an opposite and equal inclined direction towards the centre of motion. This effect, M. Poiteau says, is simply due to the exertion of a physical law, namely, that when a heavy body is projected into space by any force, as the centrifugal force, in the experiment under consideration, the heavy extremity precedes the lighter. Now, the radical extremity is of greater specific gravity than the leaf end of the plant, and consequently, in the wheel ex-

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periment, tends outwards. The experiment, therefore, has nothing to do with the natural direction of roots and stems.

Another theory considered is, that which views a tree or plant, not as a simple individual, but as a union of universal independent parts, arising each from a bud; the bud being then considered as a seed, which, under developement, produces latent roots that descend, or tend to descend, towards the earth, and constitute the fibres of the wood. In support of this theory, M. Poiteau quotes the *Rhizophora*, growing on the borders of the Mahuri at Cayenne; the trunks of which produce roots at different heights, and are obliterated at the parts inferior to these roots, because, according to M. Poiteau, the fibres of the lower buds no longer descend to the bottom of the tree, but become and constitute these aërial roots. The same is the case nearly with the *Ludovia funiculifera*, a plant of Guiana, described by M. Poiteau. Many palms have their stipes sustained by aërial roots, of which the most recent are the lowest: to explain this fact, M. Poiteau remarks, that the growth of the plant is from without inwards, from which he concludes that the youngest woody fibres, being situated within, their prolongation should be less than that of the more ancient fibres.—*Bull. Univ.* D. xiii. 74.

2. *On the Nature and Character of the Potato Root, and other Vegetable Bulbs.*—A particular investigation of the internal and external organization of the *Solanum tuberosum* and *Helianthus tuberosus* has been undertaken by M. Turpin, in consequence of which he has been led to lay down the particular distinctive characters of the stems and roots of plants, and to class the two tubers above mentioned with some others as true vegetable stems.

The essential character of roots, in whatever medium the latter may be developed, is the entire absence of vital nodes, or of those points generally expanded and disposed symmetrically, and in determinate situations on the surfaces of stems, and consequently of the foliaceous appendages which always accompany buds. The multiplication of their fibres is always adventitious, has no determinate regularity, and may take place from any part of their surface.

The essential character of stems, whatever the medium in which the latter are developed, is the occurrence of vital nodes, disposed symmetrically, and constantly bordered, or accompanied by a foliaceous appendage, which is sometimes reduced to its rudiments, or, indeed, almost absorbed. Buds and bulbs grow from these vital nodes, which are their true receptacles.

A long and accurate series of observations are then gone into, by which it is shewn, that the roots of the descending parts, and the adventitious fibres of the potato, never do, under any circumstances, thicken, so as to produce a tubercle, such as could be called a potato; and that the same is the case with the *Helianthus tuberosus*, or *Topinambour*.

True stems, which arise from the lowermost vital nodes or buds

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upon the mother stem of the plant, when they travel and lengthen themselves under ground, and become thickened at their extremities, produce the tubers of the potato and *topinambour*. The potato, like a stem, can ramify, and has produced successive ramifications below the surface of the ground up to four generations, all of them apparent, and produced one upon another. These four generations are rigorously comparable to the branch of a tree, on which were the shoots of 1826, 1827, 1828, and 1829.

M. Turpin then describes three sorts of existences, or modes of being, or three individual systems, which are observed by the microscope, as conjoining to form, by simple agglomeration, the substance of a potato. The first existence is found in the prodigious quantity of distinct mother vesicles, white, soft, transparent, and nearly spherical, which, being placed in an irregular way, with respect to each other, have insignificant and irregular spaces between. The mass of these vesicles is called, in vegetables, the cellular tissue, and in potatoes is, in fact, the starch. In those which are ovaria, exist others, representing the future cellular tissue; the latter are called globuline. The second system consists of certain internal fibrils, either simple or compound, which originate, extend and vegetate amongst the cellular tissue; these constitute the vascular tissue. The third system exists in the general membrane which invests the whole, and incloses, but without limitation, the vesicles and the fibrils: this is the cuticular system.

These three existences have a perfect individuality, and never change their nature; but the cuticle is remarkable, for that nature seems to have placed in it the power of restraining, in every way, the blind and uncertain developements of vesicular or vascular systems which vegetate beneath it.

Each vesicle of the cellular tissue, and each fibril of the vascular system, has its particular vital centre of vegetation. Each of these elementary systems lives, increases, and propagates upon its own account; at the same time that it is subjected to make part of a more compound individuality—namely, that of the plant.

This multiplicity of particular lives, or distinct individuals, in the composition of the masses of vegetables, explains how the life of a plant is equally spread over the whole of the living part; and how from any of these points may be developed the germination of an adventitious embryo, and consequently of a new plant.

M. Turpin then applies similar reasoning to animal systems, and endeavours to shew there, also, the existence of independent vital centres, or, of individualities which, though quite distinct, are made to hold the place naturally belonging to them in the complicated animal system; and in thus viewing the subject, he says, "I think I have touched the most important point in the organography and physiology of organised beings."—*Mém. du Muséum*, xix. p. 1.

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3. *Peculiar Cultivation of Potatoes.*—A French soldier placed half a dozen potatoes at the bottom of a cask upon a layer of sand and fresh earth, three or four inches thick; when the stalks had risen a few inches, he bent them down and covered them, four or five inches deep, with the same mixture. He continued this operation until the cask was full. Six or seven months after, upon emptying the vessels, (which stood in a court-yard,) he found that the half dozen of potatoes had produced an enormous quantity of new ones from the portions of the mother stems which had been successively laid down and covered.—*Jour. des Connais. Usuelles*, 1829, p. 66.

4. *Curious Phenomenon in Vegetable Physiology.*—M. Alix, of St. Valery, near Somme, has in his possession an apple-tree the source of which is unknown, and the age supposed to be forty years. This tree, which perfectly resembles the ordinary apple-tree in its leaves, and the disposition of its flowers, differs by the flowers being deficient of petals and stamina, but possessing instead fourteen styles and a calix with ten segments, connected below, but disposed in two alternate ranges. The peduncle of the flower is woolly, and the styles, being slightly hairy at the base, are surmounted by a very viscid oblique stigma. In consequence of the organization of these flowers, the tree was sterile, until, it having been suggested that artificial fecundation could be effected by means of pollen taken from other apple-trees, the tree was made to produce fruit. Since then, it has become a sort of festival in the country to render the tree productive; and those of the neighbourhood who feel an interest in the progress of the tree, when it comes into flower, so soon as they meet with a perfect apple flower elsewhere in fine dry weather, they pluck it and apply it to one of the flowers of the sterile tree, and leave it there until it falls off; then the person distinguishes the flower by attaching a coloured ribbon to it, so that in the autumn each may know his apple.

The apples differ much from each other in their size, taste, and colour, because of the variety in the trees from which the perfect flowers were taken; but they are all distinguished by a degree of contraction, situated nearly about one-third from the end. Within each apple are fourteen cells, situated in two horizontal and parallel planes; five of these are disposed as in the ordinary apple, in the middle of the fruit; but the other nine, which are smaller, are nearer to the top of the apple. Each cell does not always contain a seed; the number of the latter varies from three to nine. The arrangement of these cells has some analogy with the appearance which two apples would present if fastened end to end, and of which the longitudinal section would present the figure of a leaf in the shape of a violin or panduriform.

Wildenow, Poiret, and others, have described unisexual apple-

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trees, in which the petals and stamina were absent; but they differ from the tree of St. Valery in their fecundation being effected by the vicinity of other apple-trees, and in having only a five-leaved calyx, from five to ten styles, and five cells in the fruit. M. Tillette de Clermont explains the present case, by the theory of junctions and miscarriage developed by M. De Candolle. In applying this theory to the tree in question, it must be imagined that there is the flower of an ordinary apple-tree, from which are developed two other flowers, which, instead of being supported on separate peduncles, must be considered as joined together, and at the same time conjoined with the simple flower from which they have their origin; and this in such a manner, that the soldered ovaria of the two upper flowers are superposed and soldered to the ovarium of the inferior flower, a style and a cell being at the same time suppressed. This natural monstrosity, therefore, is the product of three flowers soldered together, in which there is at the same time suppression of the petals, stamina, a calyx, and a pistil. The examination of the fruit leaves no doubt on this subject, and evidently proves the truth of M. Clermont's hypothesis.—*Revue Ency.* xliii. 762.

5. *Effect of Iodine upon Germination.*—A series of comparative experiments have been made by M. Cantu, upon the germination and vegetation of plants moistened with water, solution of chlorine, and solution of iodine; the latter of equal density. The following are his conclusions: i. Iodine is generally more effectual than chlorine in facilitating the germination of seeds; ii. Iodine produces this effect by stimulating the germen of the seeds in the same manner as oxygen and chlorine; iii. Iodine is absorbed by the growing plant, but, by its affinity for hydrogen and the power of vegetation, is soon converted into hydriodic acid; iv. The germination of seeds, which appear to have lost all vital power, may frequently be excited by iodine.—*Bull. Univ.* D. xii. 74.

6. *Preservation of Seeds.*—M. D'Arcet has preserved corn, which had been infested by weevils, for a considerable time, by putting it into vessels previously filled with sulphurous acid. All the weevils perished, and the corn ceased to suffer. In this manner insects in seeds may not only be destroyed, but their presence prevented. As it might be inconvenient to burn sulphur in the vessels to be filled with sulphurous acid, we will indicate another method of replacing the acid, and obtaining the same results. All that is necessary is to powder the seeds well with flowers of sulphur, before they are put into the bottles or other vessels; or, after having put the seeds into a bottle, the sulphur may be added, and the whole well shaken together, so as to bring it into contact with all the seeds. The presence of the sulphur will prevent entirely the attacks of insects.—*Journ. des Connais. Usuelles*, 1829, lxviii.

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7. *New and hardy kinds of Olives.*—Two new species of the olive have been discovered in the southern district of the Crimea; this discovery will render it practicable to rear this useful tree in much more northerly climes than has been hitherto possible. The shoots, which were planted in the botanical garden of Nikita, have lived through one of the hardest winters ever known, though the severity of the weather would have been fatal to the Italian or French olive.—*New Monthly Mag.* xxvii. 438.

8. *On a dangerous Plant growing among Water-Cresses.*—The procumbent water parsnep, or *Sium Nodiflorum*, is a dangerous plant of the umbelliferous class, which grows mixed with water-cresses in springs and streams; when not in flower it so much resembles the latter, that it is with difficulty distinguished except by a botanist. Water-cresses are of a deeper green and sometimes spotted with brown, and the extremities of the leaves are more round, and especially the last leaves, which are in pairs larger than the others, and undulated at their edges. The water-parsnep, on the contrary, is of an uniform green, the ends of its leaves are longer and narrower, conical at the extremities, and toothed at the edges. The best method of knowing them well is, to examine them in July, when their flowers are expanded, and when they may be thoroughly distinguished from each other.

9. *Habits of the Egyptian Scarabæus.*—From the Notes of a Traveller in the Libyan Desert: "October 12th. Being on watch this night, I caught, for the first time, the *Scarabæus ateuchus sacer*, or chafer, with which the imaginations of the ancient Egyptians so frequently busied themselves. My attention was attracted by a noise close to my side, and athwart the darkness I discovered a large rolling ball. Conceiving it to be a crab or land tortoise, I took it into my hand, but found it to be nothing but a lump of horse-dung, and immediately afterwards I perceived a similar ball come rolling towards me. Upon holding my lantern down and minutely examining this strange machine, I found that it concealed a large black chafer, who drove it forwards by means of his long hind legs; and as it proceeded it gradually increased in size by the continual accumulation of sand; this indeed became so considerable at last, that the insect itself was scarcely perceptible. It is more than probable, that the Egyptian priests took advantage of this deception to mystify their followers, and that their veneration for the chafer, or scarabæus, arose from that circumstance. Upon a further examination, with the aid of my lantern, I discovered several animated balls of a like description more than three inches in diameter. My Arabian companions, however, did not appear to take notice of them."—*N. M. Mag.* xxvii. p. 482.

10. *Method of killing Insects for Preservation in Cabinets.*—This

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method consists in inclosing the insect in a paper or thin wooden box, and exposing it for one or two seconds to heat near the fire. The heat immediately kills insects the most tenacious of life. The process does not alter the most delicate colours; but if the heat be continued too long, the wings and other parts of the body begin to wrinkle.—*Bull. Univ.* B. xviii. 312.

11. *New Applications of Chloride of Lime.*—This substance is daily finding new applications. In France, a very important one appears to be, its application to silk-worm houses. A moderate use of it in some very strong cases appears to have preserved the worms in health even when they were purposely put in contact with diseased worms. As an ordinary application, it seems to be sufficient to put about an ounce of the bleaching powder into two pints of water for each quantity of worms obtained from an ounce of eggs. This is to be put into a pipkin in the middle of the place where the worms are, the mixture agitated, the clear liquor withdrawn, and sprinkled about the place two or three times in each twenty-four hours, according to the state of the air. Fresh water is to be added in a similar way to the undissolved portion, which is not to be rejected until it has lost its peculiar smell.

12. *Preservation of Meat and Fish by Means of Ice.*—Some experiments have been made by order of the Council of Health of the Prefecture in Paris, relative to the preservation of meat and fish by means of ice. The experiments have been varied, both as to the time during which the application of cold was continued, and the nature and state of the substances operated with. The results were as follows:—i. Fresh meat of every sort, and also fish, may be preserved in ice for a long time without suffering any alteration. ii. The placing these substances in ice, when in a putrefactive state, will stop the decomposition. iii. Substances which have been put in a fresh state into the ice, and kept there for a longer or shorter period, when withdrawn and exposed to the action of the air putrefy with great rapidity; if the temperature is rather considerable, even a few hours will suffice to bring on putrefaction, and render the substances unfit for food. iv. When these substances are cooked immediately after being taken from the ice, they not only do not lose any of their good qualities, but are more tender and delicate than if they had not been so preserved.

13. *Torsion of the Arteries.*—M. Amusat has lately described to the Academy of Medicine at Paris, a process which he proposes to be used in place of ligatures to the arteries and veins, when these vessels are divided. The method consists in subjecting the artery to torsion, and has succeeded beyond his expectation. It is as follows:—When the artery has been exposed and cut, its free extremity is to be laid hold of by a small pincers, which close on

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it by a spring; then applying more or less force, the artery is to be drawn until five or six lines in length have been separated from the flesh; then, by means of a second pincers, the surrounding tissues are to be forced upwards or downwards, so that the artery shall be perfectly isolated from the nerves and other soft parts, which might have been laid hold of with it. That done, a rotatory movement is to be impressed on the artery by revolving the pincers between the fingers, and this is to be continued until the portion held by the instrument is broken; the hæmorrhage will then be arrested. When an artery is to be twisted, it is well to fix it between the finger and thumb of the left hand, and then four or five turns will be sufficient to break it. When it is left free, the torsion extends up it to the next branch, more turns are required, and the operation prolonged. When the twisted artery is left to itself, it becomes tense and vibrates in accordance with the pulse; when dissected, the twisted part is found to be solid, and the inner coats are found to be broken, as in the application of a ligature; but besides they are puckered, and form a sort of *cul-de-sac*, or valve, against which the blood presses.

M. Amusat has made many experiments upon this mode of closing arteries, with great success. He has applied it twice in human beings; once in a case of amputation at the knee, and once in a case of extirpation of the testicle; in both it was perfectly successful. In his experiments upon dogs, he has tied the crural artery on one side, and twisted that on the other: two of the animals died from hæmorrhage occurring on the side where the ligature had been applied. One advantage urged by M. Amusat in favour of the process is, that the artery is always operated upon free from the nerves and neighbouring parts, whilst ligatures often inclose these parts and produce serious evils; he adds, also, that ligatures are rarely drawn tight enough, or well practised. He thinks that the process may be substituted, with advantage, for that of tying; i. Because it is simpler; ii. Because a single person can do it, requiring no assistance—an advantage of great importance in pressing cases, either in the country, or in battle; iii. Because it offers great advantages in the immediate re-union of the parts affected.

M. Amusat's process has, however, been very much canvassed, and opposed by other members of the Academy; some stating that it was difficult of performance, and occasionally dangerous; others that it was old, and others that M. Amusat's observations upon the process of tying were not correct.—*Bull. Univ.* C. xviii. 459.

14. *On Metallic Ligatures applied to Arteries.*—Mr. Lerut has lately been led to ascertain the value of a suggestion thrown out some years ago by Dr. Physick, of using leaden ligatures. The idea arose from considering, that in numerous cases, bullets, buck-shot and lead, would remain in contact with almost any tissue of the body,

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without producing irritation or unpleasant consequences, and that for an indefinite period.

Mr. Lerut laid bare the right carotid artery of a dog, and after separating it carefully from its accompanying nerve and vein, passed under it a leaden wire, which was then firmly tied. Both ends of the wire were cut off, and the sharp point bent down. The wound was then drawn together by a few stitches and adhesive strips. The animal was left at liberty, and being examined after some days, the stitches were found ulcerated out and the wound open; it had filled up from the bottom with granulations, but the edges were wide apart. With tight dressing it healed entirely, in about ten weeks. A few weeks after, the animal was killed and examined: a small cicatrix existed in the skin; the lead was found in the situation in which it had been placed, by the side of the vein and nerve, perfectly encysted. The artery had been removed entirely, for the space of half an inch. Not the slightest trace of inflammation existed in the neighbouring parts; on the contrary, they appeared perfectly natural. The lead was inclosed in a dense cellular substance, which formed for it a complete cyst.

In four other similar experiments, not the slightest departure from the former appearances occurred. In every case the lead became inclosed in a cyst, and the neighbouring parts remained perfectly healthy and natural. The lead having answered so well, the experiments were continued to ascertain whether that metal was peculiar in this respect, or whether other metals were as innocuous in similar circumstances. Trials with gold, silver, and platinum had exactly the same results; from which Dr. Lerut concludes, that the plan of tying the arteries with lead and the other metals is free from danger, and may be productive of some peculiar advantages.—*Amer. Journ. Med. Sciences.*

15. *Metallic Silver in the Animal Tissue.*—A person who had taken nitrate of silver for epilepsy, was cured, but afterwards died of a diseased liver. His skin had, however, previously acquired the bluish tint now so well known as communicated by the medical use of nitrate of silver, and therefore, after death, the body was examined: it was found that the internal parts, also, had undergone more or less change in colour, as well as the skin. M. Brande undertook the chemical examination of the plexus choroïd and the pancreas, and found both to contain notable quantities of silver.

16. *On the Ergot of Mais, or Indian Corn.*—M. Roulin has written on this subject, and his memoir has been reported upon to the Royal Academy of Sciences. This ergot does not resemble that of rye in its appearance, but produces similar effects. Pigs who feed upon it, lose their hair, and their posterior limbs frequently become paralyzed. With mules the hair falls off, the feet swell, and they frequently lose one or more hoofs; the latter are, gene-

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rally, reproduced when the animals are left in pastures. Poultry nourished by such grain, often lay eggs which are without shells; and M. Roulin conjectures, because the ergot causes a convulsive contraction of the oviduct, which expels the egg before there has been time for the calcareous matter to be secreted upon its surface. This disease in the mais is not known, and does not exist in Mexico or Peru; and when grains attacked by it are conveyed beyond Paramos, or the regions of eternal snow in the Cordilleras, they may be used without inconvenience or danger.—*Revue Ency.* xliii. 769.

17. *Poisoning by Strychnia.*—M. Guibourt stated lately to the Academy of Medicine, that having observed a dog in violent convulsions, in consequence of eating one of the compound balls, containing strychnia, or vomica-nut, which the police use to destroy wandering animals, he forcibly made it swallow powdered nut-galls, when the muscular convulsions immediately ceased; ipecacuanha was then given to the animal, but the latter could not vomit; the next day milk was given to it and manna; after which the dog recovered. M. Caventou said, that the infusion of galls was a very effectual opponent to vomiting, and that he had observed it destroy the power of emetic tartar. M. Orfila has already advised the administration of this infusion, in cases of poisoning by opium and salts of morphia.—*Bull. Univ.* C. xvii. 60.

18. *On Vegeto-Alkaline Poisons, and the Neutralization of their Power.*—In a memoir, read by M. Donne to the Academy of Sciences, he states, that morphia, brucia, strychnia, &c., combine with chlorine, iodine, and bromine, to form distinct compounds. These are true chlorides, iodides, and bromides, and may be decomposed by the acids, and the vegeto-alkalies separated. It appears also that these compounds are innocuous in comparison with their bases, and that the compounds of strychnia, when given in doses of 2½ grains, produced no effects on a dog, whilst half a grain of pure strychnia killed a dog of much larger size.

Experiments were then made to ascertain the power of chlorine, iodine, and bromine, as remedies against the poisonous properties of the substances mentioned; and it was found, that if any of these substances were injected into the stomach after strychnia, and in such period of time as had not allowed of sufficient absorption of the latter to produce death, then harm was prevented. In seven experiments of this kind, where doses of one and two grains of veratria had been given, the animals were saved from death by administering tincture of iodine. Once death took place when the antidote was given eight or ten minutes after the poison; and in another case the tincture of bromine given immediately after the administration of a grain of strychnia, failed to save the animal.

When the innocuous compounds of strychnia were decomposed by

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sulphuric acid and the sulphate formed given to a dog, it killed him in less than an hour. Most of these compounds crystallize regularly, and have particular characters. They are all decomposed by acids. —*Bull. Univ.* C. xviii. p. 289.

19. *Impure Common Salt in France.*—An account has been given to the Academy of Medicine of certain impurities in common salt. M. Cosmeuil, of Rheims, had written to M. Planché, stating, that the salt used at Fere-Champenoise and the neighbourhood had caused serious injury to the inhabitants, and that, in a population of 2400 persons, 400 were ill. These patients were afflicted with violent cholic, accompanied by swelling of the face. M. Cosmeuil examined the salt, and, according to his experiments, found in it bromine, bromide of sodium, iodine, and iodide of potassium.

At the same sitting M. Laugier said, that whilst preparing muriatic acid, in one of his lectures, from common salt, he obtained a large quantity of iodine upon the addition of the sulphuric acid of commerce. M. Orfila also had met with the same circumstance.

Several of the members have been charged with the examination of the salt used in Paris. M. Baruel had met with some containing iodine. His process of detection consisted in diluting a little flour paste with water in a glass, adding to it a drop of chlorine solution and a drop of sulphuric acid, and then putting some of the suspected salt into the mixture. If iodine or its compounds were present, a more or less intense blue or violet colour was produced.

M. Chevallier had examined many specimens of salt which had been seized, but had not found iodine in any of them. Some of the specimens had been adulterated by the admixture of sulphate of soda.—*Bull. Univ.* C. xviii. 472.

20. *Application of Iodine to Scrofula.*—The following is part of a report by MM. Majendie, Serres, and Dumeril, on a mémoire by M. Lugol: "M. le Dr. Lugol has treated 109 scrofulous cases, at the Hospital St. Louis, within 17 months, with iodine alone; at the end of the last year 39 still remained under superintendence, 30 had left the hospital very much improved, 36 had gone away completely cured, and in four cases only did the remedy seem quite inefficacious. The author concludes, from this mass of experience, that iodine should be considered as the most powerful and efficacious remedy in scrofulous cases, since it has constantly arrested the progress of the disease, or at least has exerted a salutary action upon the tuberculous tumours where it has not decidedly produced their cure; and therefore, that in that point of view only its introduction into medicine is one of the most valuable acquisitions the healing art has made in late times." The reporters say they are able to bear witness to the curative power of iodine in these cases, and consider the memoir and exertions of Dr. Lugol as highly useful.—*Revue Ency.* xliii. 767.

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21. *On Sulphur found in Gypsum.*—Whilst digging a well at Malvezy, near Narbonne, sulphur was found in soft masses of a clear yellow colour, light, having a scaly structure, adhering to the tongue, and taking a good polish by friction. The gangue of this sulphur was a hard bluish fragile clay, containing traces of bitumen. The sulphur contained about a tenth of bitumen and carbonate of lime. No sulphur was found in the quarry of marly gypsum at Malvezy, but the recent gypsum always possesses a decided odour of sulphuretted hydrogen, and by exposure to air becomes covered with an efflores rence of sulphate of soda. The two strata of blue marl which traverse the quarry are full of sulphate of iron.

The sulphur of Malvezy is found beneath the gypsum; the blue clay containing it is connected with the gypsum, and thus makes part of the lower system of the second fresh-water formation. Sulphur, having the same characters as that of Malvezy, has been found at the old plaster works near Vidilharn.

The particular state of this sulphur shews, that it is the result of a deposite formed by the decomposition of some mineral sulphuretted water. I believe it is certain that this formation of sulphur lies upon the first fresh-water formation (lignite and plastic clay).—M. Tournal.—*Journ. de Pharmacie.*

22. *Falls of the Niagara.*—The American papers state, that part of the great fall has gone down into the chasm below, to the extent of about an acre of the rock, on the Canada side. The curve called the Horse-shoe has been thus much extended. The Table rock is not injured, but immediately above it, in the shoe of the falls, the range has become much more straight. The launch took place at nine o'clock in the evening of the 28th December last.

23. *Thawing Power of a small Stream of Water.*—M. Huber Burnand went to visit the glacier of Grindenwald; and the vault in it, out of which flows the black Lutschine. This vault far surpasses that from which the Aveyron flows, and was estimated at 50 feet in height, and the front of the glacier there at 300 feet. The torrent is of considerable size, but at the time of the visit part of the bottom of the vault was dry. On entering the vault as far as the river would permit, day-light was seen to pass through the roof, and at the place was an enormous tube of ice hanging from above, and ready to fall. The cause of the tube, and the appearance of day-light, very soon appeared; for on leaving the vault and climbing by ladders and steps up the outside of the glacier, so as to arrive at the top and outside of the icy structure, and about 150 feet above the river below, a cavity or crater was seen in the ice, which, upon examination, proved to be a perforation formed by the action of a small stream of water, heated by the sun, and descending from the superior part of the glacier. This was a natural illustration of the fine idea of M. Venetz, applied in the Valley of Bagnes.* The stream

* *Quarterly Journal of Science*, O.S., vol. xv. p. 390.

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was not larger than that of a common fountain, but had so well fuzed and pierced the ice, that it now fell through the roof of the vault into the stream below. The cavity formed, which had probably been enlarged by the current of the air, was a little conical at its orifice, and then vertical and cylindrical apparently to the vault. Its diameter at the mouth was perhaps 24 or 30 feet, but a little way down 12 or 15 feet. It was through this that the light had entered the cavern beneath, and the tube of ice which appeared below was probably a ring of ice detached from the bottom of the well. M. Burnand thinks that the crater will go on enlarging, and that gradually the vault, out of which the river issues, will be separated from the glacier, and stand like an enormous arch alone.

After this he proceeded to a very beautiful natural icy cavern formed in another part of the glacier. The place was perfectly clear, and illuminated by the light which passed through enormous masses of ice; the ice was of a greenish colour. The place was of extreme beauty, and presented a view at each end over the neighbouring country of the most extensive kind. It was concluded that a few weeks of sunshine would destroy the whole.—*Bib. Univ.* xlii. p. 112.

24. *Meteorological Influence of Terrestrial Electricity.*—This subject has been treated of by M. Carlo Matteuci, of Bologna, who endeavours to found certain explanations of natural phenomena upon the supposed accumulation of electricity upon the surface of the earth. He considers that there is accumulation of this power upon particular localities, the electricity itself being developed by evaporation, or other circumstances upon the surface, or by internal chemical action, and when developed, being retained in particular situations by the non-conducting power of the neighbouring earth. This non-conducting power is supposed to depend either upon the particular nature of the ground, or upon its becoming dry by evaporation, and therefore, it is said, it is rather upon elevated and isolated places than upon plains, above rocks than over forests, in summer than in winter, and in the middle of the day than during the night, that those stormy clouds are formed which frequently can only be explained by terrestrial electricity.

One explication furnished by this theory, is considered as ingenious; it applies to those luminous appearances which so frequently occur in the atmosphere during the evenings and nights of summer, and are called heat-lightning. These are attributed to electricity produced and accumulated as already mentioned. After sunset, the vapours which condense, form a conducting stratum near the surface, which serves gradually to re-establish the electric equilibrium between the earth and the atmosphere. It is especially in plains, that these flashes are observed, because the electricity accumulated on high and isolated places escapes rapidly in conse-

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quence of their form, lower temperature, and the greater rarety of atmosphere about them.

Sometimes these discharges are supposed to be very powerful; and when the earth and air are very dry, are thought capable of producing earthquakes such as occur after dry seasons; and a very ancient method of avoiding such earthquakes is quoted and approved of, namely, the introduction of long bars of iron into the ground to considerable depths. These, it is supposed, serve to conduct off the accumulated electricity. The *Bibliothèque Universelle* very properly remarks, that it is desirable this accumulation of electricity upon the surface in the manner described, and upon which the whole hypothesis rests, should be proved in a decisive manner.—*Bib. Univ.* xlii. 8.

25. *Petrified Tree in the Isle of Portland.*—This petrified tree was found in the western quarries, nearly half a mile from the seashore, and as near as can be judged, about 200 feet above the level of the sea, and about 10 feet below the surface: these petrifactions are found in a sort of bed, or layer of black mould, which in some part appears like burnt wood-ashes, and is from 1 to 2 feet in thickness between the beds of stone. The bed of stone above it is about from 1 foot to nearly 2 feet in thickness, and above that bed, up to the surface, is composed of shingles and slate stone, which is very hard, and is made use of for covering the roofs of houses. The body of the tree is now of an oval form, and is as hard as flint; just above the root it is about 4 feet in circumference, and diminishes to about 2.9; it has the appearance of oak by the grain and knots: it was lying horizontally, and cracked in several pieces, which some think was from the weight on it; but Mr. Beale, a gentleman who has written on this subject, saw it, and affirms it must have been broken by contraction, and that the whole masses of stone must once have been in a fluid state, otherwise a bed of stone could not have been above it.—[Through the kindness of Governor Penn, we have received specimens of the above, which are at the Royal Institution.—Ed.]

26. *Brandy an Antidote to Beer, by* M. Recluz.—During a botanical excursion in the neighbourhood of Lyons, M. Recluz met, in a tavern, a man who was intoxicated from drinking beer, and requested the hostess to give him something to effect his recovery; the latter told M. Recluz that she had nothing but some orange-flower water, of which she put two or three spoonfuls into his mouth. Two minutes afterwards, the drunkenness continuing, M. Recluz himself administered to him some more of this liquor, when he discovered that the bottle, which was labelled orange-flower water, contained only brandy. To repair this mistake, he sent for an emetic, but in the interval the intoxication went off, and the man said he seemed to have awakened from a long and painful dream. M. Recluz has let no opportunity pass of trying the efficacy of this

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curious remedy, and always with success. M. Taillet, a physician at Agle, in the course of his practice, has also verified it, and as the subject is not mentioned in any work upon medicine, it has been inserted in the *Annales des Sciences d'Observation.*

27. *Different Methods of preserving Animal and Vegetable Substances designed for Scientific Collections.* By M. Barpay.—The advantages and inconveniences which attend different liquids which have been hitherto employed to preserve anatomical preparations, or individuals of the animal and vegetable kingdom, which enrich our museums, are well known.

Alcohol, which in travelling is not always at hand, involves expenses beyond the reach of private people. This menstruum dissolves the fat and saccharine parts, and the colouring matter, consequently, cannot always meet the views of the collector.

The solution of alum and of nitrate of potass only fulfil their destination imperfectly, and alter the tissues.

Corrosive sublimate, beside the dangers attendant upon the use of it, does not preserve better the entire substance.

Mr. John Davy* has proposed the solution of sulphurous acid gas, which is subsequently filtered, to give transparency to the liquid. This preparation unites, to the lowness of its price, the property of preserving substances for an indefinite time, and of rendering transparent the least compact parts of the organization. In this manner, the author has preserved for three years several anatomical specimens, which are as fresh as at the time of their immersion; the bottles are closed, corked, and luted with wax. The same process, Mr. Davy assures us, may be pursued with regard to vegetable substances. The specimens in pathological anatomy may also be preserved uninjured in the same liquid.

Care must be taken to plunge these substances into the solution before putrefaction has commenced. If it be wished to render the structure of the tissues more apparent, a strong solution must be employed. A weak solution only is required, when anatomical specimens are to be preserved.

This process certainly will not answer for substances of which it is wished to preserve the exact form and colours.

M. Vignal, who has the care of the botanical specimens of the Faculty of Medicine of Paris, has discovered an excellent method of preserving animal substances; it is, to plunge them in water containing an excess of camphor in lumps. As long as the lumps of camphor remain in the water, the specimens are preserved without alteration, even from the contact of air. They would be preserved for an indefinite time in a bottle with a ground glass stopper. M. Vignal, from whom may be expected more ample details on this subject, has kept a fœtus two or three months, or for more than a year, in an open vessel.

* Transactions of the Medico-Chirurgical Society of Edinburgh, vol. iii. part 1.

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These solutions do not appear suited for the preservation of vegetable substances, still less for that of mushrooms.

In 1827, M. F. Leudersdorff* has recommended the use of fat oils mixed with alum or cream of tartar. The alum should be previously separated by strong heat from its water of crystallization. It then penetrates the plants almost as soon as the oil, absorbs all their humidity, and preserves their colours entirely. It is only necessary to steep the plant in this mixture, and then place it between some gray paper for twenty hours, that the oil may penetrate it entirely; afterwards lay it in some dry paper, and in a short time it will be ready for the herbal. When travelling, the plants may be left in a vessel full of oil; the care of drying them being left till you return. As for mushrooms, the author plunges them into mutton suet exposed to a heat of from 42 to 45° R., after having pierced the epidermis of the mushrooms in different points. The suet penetrates into the substance of the mushroom, and what is over is removed when cold; in this way the mushroom retains not only its form, but also its colours, &c.

The chemical composition of mushrooms made me think, for a long time, that an infusion of gall-nuts might be substituted with advantage for pyroligneous acid, which is ordinarily employed to preserve this species of cryptogamous plants. The pyroligneous acid alters the colours, and the consistence of the mushroom. The infusion of gall-nuts with a slight quantity of alcohol, it appeared to me, should obviate these inconveniences, and experience has not contradicted my conjecture. M. Petit, to whom I communicated the fact, has thus preserved, during two months at least, and without any alteration, the most perishable mushrooms. Far from becoming soft, these individuals had acquired in the liquor a greater consistence, and they had perfectly retained their colours. The vessel, however, was simply covered with a cork, and a considerable quantity of mould was upon the surface of the liquid. Care should be taken that the mushroom be entirely immersed in the solution.—*Annales des Sciences d'Observation.*

28. *On the vegetating Wasp of Guadaloupe, by* M. J. B. Ricord-Madianna.—Botanists and entomologists know that particular productions which have been recognised as cryptogamous plants, many of which have been referred to the genus Sphæria, are frequently met with on dead insects, and are preserved in collections; but it has been thought that these plants developed themselves on insects deprived of life. M. Ricord, however, states, that he has observed at Guadaloupe a nest of wasps, the greatest number of which were encumbered with these excrescences. As they quitted the nest, they fell upon the ground, and could not rise again on account of the weight of the plant, which had taken root on some part or

* Das Austrocknen der Pflanzen, &c. 8vo. pp. 150. Berlin, 1827.

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other of their body, particularly on the sternum. Having observed the larvæ contained in the cells, M. Ricord remarked, that they also had this small cryptogamous appendage, but then it was very small. This species appears to be the *Sphæria entomorbiosa* of the English botanists.—*Journal du Pharmacie.*

29. *Artificial Incubation.*—M. Lotz has stated in the *Iris*, that the greatest difficulty in artificial incubation is, to graduate the heat properly; for which purpose he has supplied the following table, the result of his own experiments.

Days.	Pheasants. C.°	Pea Fowl. C.°	Guinea-Fowl. C.°	Partridge. C.°	American Duck. C.°
1	5	5	5	5	5
2	10	10	10	10	10
3	13	13	13	13	13
4	16	16	16	16	16
5	19	19	19	19	19
6	22	22	22	22	22
7					
8	25	25	25	25	25
9					
10	30	30	30	30	30
11					
12					
13	33	33	33	33	33
14					
15					
16	38	38	38	38	36
17					
18					
19	38	38	38	38.5	36
20	38.5	39	39	39.5	36
21					
22	38.5	39	39	39.5	37
23					
24	38.5	39	39	39.5	37.5
25		39	39.5		
26		39.5			

30. *Note relative to some blistering Insects, by* M. Farine.—It has been long known that cantharides are not the only insects which possess a vesicatory property, and that it is common to many other genera. From some comparative experiments, M. Farine has ascertained, that the *mylabris cyanescens* is, after the cantharides, the insect which possesses this property in the highest degree; the *mylabris variabilis* follows. The author has also determined, that their action is the stronger when they inhabit warmer places, and more exposed to the sun. The male *meloe majalis* is always more

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epispartic than the female, and this property is more active in proportion as it is more quickly killed, for if it be kept alive only a few hours it sensibly diminishes. In the same genus the species are more or less vesicatory. The *meloe autumnalis* is less so than the *majalis*, and more than the *meloe reticulata*, and the *meloe tuscia* is infinitely less. The *rissiphorus bimaculatus* and *flabellatus* are without action, while the *rissiphorus subdipterus* is slightly epispartic; the *zonitis præusta* is innocuous, and the *zonitis punctata* is sensibly active. The period of coupling seems to be that in which this faculty is most developed. The author proposes during this time to collect them, but precautions will then be necessary to prevent the collection made being injurious to the produce of the ensuing year.—*Journal du Pharmacie.*

31. *Ratio of the Births of Males and Females relative to the Age, &c. of the Parents, by* Professor Hofacker.

i. In marriages, where the mother is older than the father, the number of boys (which generally is in the ratio of 101 : 100) is to that of the girls :: 90·6 : 100.

ii. When the parents are of the same age, the ratio of the boys to the girls is 92 : 100.

iii. If the father be from 3 to 6 years older than the mother, 103·4 boys : 100 girls, nearly the ordinary proportion in Europe.

iv. If the father be from 6 to 9 years older than the mother, boys : girls :: 124·7 : 100.

v. If the father be from 9 to 12 or more years older than the mother, boys : girls :: 145·7 : 100.

vi. If the father be 18 years and upwards older than the mother, boys : girls :: 200 : 100.

vii. Young men, from 24 to 36 years of age, with young women from 16 to 26, change the ratio to 116·6 boys : 100 girls.

viii. Young men, with older wives, between 36 and 46 years of age, have boys : girls :: 95·4 : 100.

ix. Men of a middle age (from 36 to 48 years) with young wives, have boys : girls :: 176·9 : 100.

x. Middle aged men (from 36 to 48 years) with wives of a middle age, have boys : girls :: 114·3 : 100.

xi. Middle aged men (from 36 to 48 years) with older wives, have boys : girls :: 109·2 : 100.

xii. Older men (from 48 to 60) with young wives, have given no determinable result, on account of the small number of observations.

xiii. Older men with wives of a middle age, have boys : girls :: 190 : 100.

xiv. Older men with older wives, boys : girls :: 164 : 100.

Many of the above proportions are directly at variance with those of M. Girou de Buzaiengues, and to ascertain the value of them, we should know the data on which they rest, and which have not been given.

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82. *On the different Colours of the Eggs of Birds, by* M. Gloger. Verand. der Gesells. Natur. Freunde in Berlin.—It is a remarkable provision of Nature, that birds, whose nests are most exposed; and whose eggs are most open, to the view of their enemies, lay eggs of which the colour is the least distinguishable from that of surrounding objects, so as to deceive the eye of birds or of other plundering animals; while birds, the eggs of which have a bright, decided colour, and are consequently very conspicuous, either conceal their nests in hollows, or only quit their eggs during the night, or begin to sit immediately. It is also to be remarked, that in the species of which the nest is open, and the female brings up the brood without the assistance of the male, these females are generally of a different colour from the male, less conspicuous, and more in harmony with the objects around. The foresight of Nature has, therefore, provided for the preservation of the species of which the nest is altogether exposed, by imparting to the eggs a colour which will not betray them at a distance, while she could, without inconvenience, give the brightest colour under circumstances where the eggs are concealed from view. Or, perhaps, to speak more correctly, numerous birds can deposit their eggs in places accessible to view, because the colour of the eggs makes them be confounded with the surrounding objects, while other birds are obliged to conceal their nests, because the conspicuous colour of the eggs would have attracted their enemies. Let the explanation, however, be what it may, the fact exists, and M. Gloger, who has examined all the birds of Germany, has satisfactorily proved it.

Eggs must be distributed into two series according as their colour is simple or mixed. The simple colours, such as white, blue, green, yellow, are the brightest, and consequently the most dangerous for the eggs.

i. The pure white, the most treacherous of colours, is found among birds which breed in hollow places, like the woodpeckers, the wrynecks, the roller, the merops, the kingfisher, the snow bunting, the robin, the water owzel, the swallow, the martins. It is only among these birds that the eggs are of a remarkable whiteness. The eggs are also white among some species which, like the domestic swallow, certain passeres, the troglodites, &c. construct their nests with such narrow openings, that the eye of their enemies cannot penetrate within. White eggs are also found with birds that quit them only during the night, or at least very late during the day, such as the owls and falcons. Lastly, this colour is found among birds which lay only one or two eggs, and sit immediately after, like the pigeons, the boobies, and the petrels.

ii. As to the bright green or blue colour, it is found to belong to many species which make their nests in hollows, like the starling, the bullfinch, the fly-catcher, &c. In the second place, this colour is common to the egg of birds, the nests of which are constructed with green moss, or placed at least in the midst of grass, but always

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well concealed, such, for example, as the tomtit, linnet, &c. Lastly, green eggs are met with among many strong birds able to defend themselves against plunderers, like the herons.

iii. A light green colour, verging toward a yellowish tint, is found among the eggs of the many gallinaceæ which lay among the grass, without making a finished nest, which soon disappears beneath the quantity of eggs; like the hoopoe, the perdrix cinereus, the pheasant. The same colour is also remarked among several of the palmipedes, which quit their eggs when they lay them, but which are attentive in watching them, as the swans, the geese, the ducks, the divers, &c. The eggs of certain great birds which make their nests in the open air, but are well able to defend themselves, are a dirty white, as may be observed among the vultures, eagles, storks.

Among the eggs of a mixed colour, they are to be distinguished which have a white ground, and those of which the ground differs from white. The eggs with a white ground are those of the European oriole, the long-tailed tit, the cole-tit, the nut-hatch, the creeper, and the common swallow. Most of the eggs with a white ground are concealed in well-covered nests. The eggs of a mixed colour, and of which the ground is not white, at least of a pure white, are those of the lark, the grasshopper-lark, the yellow hammer, the wagtail, &c.; then the crows, the jays, the thrushes, the quails, &c., with most of the singing birds, the colour of the interior of whose nest harmonizes with that of the eggs*.

* We have inserted an abstract of M. Gloger's paper, from the attention which his hypothesis seems to have met with from continental naturalists. For ourselves, we have been led to conclude that he is among the number of philosophers who first imagine a system, and then would elaborate facts to support it. The rooks, for example, build a nest particularly exposed on the highest trees; the jackdaws conceal theirs in holes, while the lapwing, woodcock, and snipe lay on the bare ground, and yet the colour of the eggs of all these birds is nearly identical: again, the blackbird and song-thrush are birds of very similar habits; they build in the same places, but the blackbird lays a dull rusty-coloured egg, and the thrush a clear blue one, with a few dark, well-defined spots. The woodpeckers, it is asserted, lay white eggs; they ought according to the theory, but their practices seem very different. The hawks, which are so able and *accustomed* to defend their nests, we should expect to find with pure white eggs, but they are dull-coloured and inconspicuous—the buzzards, the most cowardly among the tribe, have perhaps the most conspicuous eggs of that tribe. The magpie is a strong bird, its eggs well concealed, and the nest fortified; but the colour of this egg is dull, like the rook, woodcock, &c. Two very similar eggs are those of the redstart and hedge-sparrow; the former builds in holes, the latter does not. The cuckoo very commonly selects the nest of the hedge-sparrow, a spotted brown egg among bright blue. Now if we admit that the brightest white eggs are to be found in birds whose nests are the most concealed, as the king-fisher, wryneck, wrens, tits, sparrows, and especially the sand-martin, may we not rather infer that, because the interior of these nests is peculiarly dark, the bright white colour is convenient to the bird, to enable her to distinguish them? At all events, we must regard M. Gloger's hypothesis as ingenious, rather than supported by facts.—Editor.

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METEOROLOGICAL DIARY for the Months of September, October, and November, 1829, kept at EARL SPENCER's Seat at Althorp, in Northamptonshire.

The Thermometer hangs in a North-eastern Aspect, about five feet from the ground, and a foot from the wall.

FOR SEPTEMBER, 1829.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Tuesday ...	1	50	65	29.86	29.83	N	NW
Wednesday.	2	53	66	29.80	29.80	NW	NW
Thursday ..	3	41	63	29.98	29.96	NW	WbN
Friday	4	39	64	29.87	29.80	W	W
Saturday...	5	42	62	29.61	29.40		
Sunday	6	47	66	29.30			
Monday....	7	42	64	29			
Tuesday ..	8	45	64				
Wednesday.	9	47					
Thursday ..	10						
Friday.....							sW
Saturday						8S	W
Sunda						WbS	SE
M					29.26	W	W
				29.56	29.48	W	ENE
				29.69	29.60	W	S
			61	29.15	29.10	SE	N
			58	29.31	29.62	NW	NW
		37	59	29.78	29.70	NW	W
	21	46	62	29.67	29.62	W	W
	22	41	61½	29.67	29.67	SW	
nesday.	23	45	63	29.67	29.71		
ursday ..	24	49	60				
iday	25						
urday...							

FOR OCTOBER, 1829.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Thursday ..	1	37	59	30.12	30.12	NE	NE
Friday	2	46	58	29.98	29.81	E	SE
Saturday...	3	53	59	29.68	29.63	W	W
Sunday	4	44	60½	29.80	29.75	W	W
Monday....	5	51	58	29.47	29.47	WSW	W
Tuesday ...	6	40	53	29.48	29.56	W	WbN
Wednesday.	7	36	45	29.46	29.49	W	NNW
Thursday ..	8	31½	49	29.63	29.90	NW	NW
Friday.....	9	29¼	47	30.01	30.18	W	NW
Saturday...	10	31	61	30.21	30.27	SSW	W
Sunday	11	49	60	30.16	30.00	W	WSW
Monday....	12	49	58	29.95	29.80	NW	W
Tuesday ...	13	47	59	29.87	29.60	W	WbS
Wednesday.	14	47	51	29.20	29.76	WNW	N
Thursday ..	15	31	49	30.03	30.13	WbN	NW
Friday.....	16	29½	55	29.93	29.68	S	WSW
Saturday...	17	44	56½	29.80	30.00	W	WNW
Sunday	18	47	61	29.92	29.97	SW	SW
Monday....	19	37	63	29.90	29.86	SW	S
Tuesday ...	20	54	60	29.73	29.64	SW	SW
Wednesday	21	42	58	29.61	29.50	SW	SSW
		44	55	29.41	29.70	W	N
		37	50	29.79	29.82	W	WNW
		34	50	29.82	29.82	WbN	N
		9	53	29.98	30.08	WbS	W
		49	50	30.20	30.20	WbN	ENE
		30	50	30.26	30.20	WNW	WbN
		43	50	30.07	30.20	NE	N
...	29	30	46	30.20	30.16	W	W
Friday.....	30	40	54	30.00	29.84	W	WbS
Saturday...	31	41	51	29.73	29.93	NNW	NW

FOR NOVEMBER, 1829.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Sunday	1	26	41	30.00	30.00	WbN	W
Monday....	2	27	49	30.00	30.03	W	W
Tuesday ...	3	29	50	30.03	29.98	WSW	SW
Wednesday.	4	40	53	29.70	29.48	S	S
Thursday ..	5	33	51	29.71	29.81	W	W
Friday	6	36	51	29.81	29.70	SW	SW
Saturday...	7	31	48	29.70	29.70	W	W
Sunday	8	29	47	29.80	29.81	W	W
Monday	9	33	47	29.81	29.88	W	W
Tuesday ...	10	34	50	29.82	29.73	SW	SW
Wednesday.	11	38	52	29.87	29.80	W	SW
Thursday ..	12	41	57	29.70	29.65	SW	SW
Friday.....	13	45	51	29.78	30.00	NE	E
Saturday...	14	38	49	30.00	29.97	E	SE
Sunday	15	37	52	29.75	29.70	W	NE
Monday	16	31	43	29.98	30.10	N	NE
Tuesday ...	17	25	41¼	30.20	30.18	NW	NW
Wednesday.	18	30	43	30.18	30.27	NW	N
Thursday ..	19	22	38	30.27	30.27	NE	NW
Friday	20	18	38	30.20	30.19	WNW	W
Saturday...	21	18	39	30.14	30.08	W	SW
Sunday	22	27	40	29.50	29.44	SW	NW
Monday	23	33	44	29.50	29.52	N	NE
Tuesday ...	24	31	41	29.60	29.58	E	NE
Wednesday.	25	29	35	29.50	29.52	NE	NE
Thursday ..	26	30	37	29.70	29.75	NE	E
Friday	27	33	40	29.67	29.62	E	EN
Saturday...	28	35	42	29.62	29.50	NE	NE
Sunday	29	37	43	29.65	29.65	SE	SE
Monday....	30	35	42	29.67	29.67	EbS	E

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London: Printed by W. Clowes, Stamford-Street.

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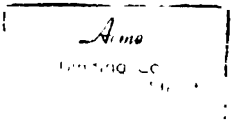

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